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===== FUSION FORUM created at 21:19:18 on 89/03/28 GMT (by CHESS at YKTVMV) ===

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NOTE : This FORUM will be pruned every 5 days to reduce its length. Data and references will stay, but questions will be deleted unless answers appear. Corrections will be incorporated into the original appends. If you are unfairly excised, just resubmit.

J. F. Ziegler

FORUM ON ROOM TEMPERATURE FUSION

On March 23, 1989, scientists at Brigham Young University submitted a paper which reported the observation of cold nuclear fusion. Their work was also circulated as a preprint. Their evidence for identifying products of nuclear reactions taking place from a bottle at room temperature is quite convincing. On the afternoon of March 23, the University of Utah held a press conference where two scientists announced they had achieved room temperature fusion. They stated they did not have a paper, and had only modest evidence of nuclear reactions.

The work of Bringham Young University is complete enough to believe that the effect they see is real and unequivocal. This achievement can be predicted to create the same intense interest as the discovery of high temperature superconductivity. The implications for future technology will create wide speculation in the popular press, and the equipment for conducting the experiments are easy and cheap to obtain.

This forum has been opened to report developments in the field of RTF (room temperature fusion) or CNR (cold nuclear reactions). We are setting up this forum on the PC disk because we know of no other way to establish contact with the diverse IBM community who might contribute to the field. It is expected that this forum will disappear within a few months once the community of interested IBM parties is established.

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J. F. Ziegler, IBM-Research, Yorktown

Room Temperature Fusion - Historical Perspective

Room temperature fusion (RTF) is simply any process which can cause exothermic nuclear reactions between atoms having no more than thermal energies. The isotopes of hydrogen are usually those evaluated. The most common reactions are d-d and d-t (where d=deuterium, one proton and one neutron, and t=tritium, one proton and two neutrons). Both of these reactions are exothermic and can occur at zero kinetic energy except for the repulsive force of the protons and the long-range repulsive nuclear force. Classically one needs keV energies to overcome these forces, but quantum tunnelling allows penetration at low energy with very very small probabilites.

The first prediction of RTF was in 1947 by F. C. Frank. He said that if one could replace the electron on one hydrogen atom with a negative muon (a particle with 200 times the mass of the electron) the orbit would be much smaller and the atoms could get much closer together and tunnelling would be easier. He estimated that fusion would occur at thermal energies.

In 1957 this effect was discovered by Alverez et al. and was called Muon-Catalyzed Fusion. Since 1957 there have been many papers evaluating the use of this reaction to produce nuclear power. Problems occur in the cost of producing muons, which require a proton beam of more than 100 MeV, and with the lifetime of muons which is only a few micro-seconds.

In 1968 S. E. Jones published an article which tried to evaluate what kind of RTF could be achieved without the use of strange particles like muons. He came up with the following predictions:

d - d Fusion at STP

d-d Separation	Fusion Rate
0.74 A	10E-70/Mol.-sec.
0.37 A	10E-20/Mol.-sec.

Deuterium gas, D2, has a normal molecular separation between nuclei of 0.74 Angstroms, and this produces one spontaneous fusion for 10E70 molecules. However, if this internuclear distance is halved the fusion rate increases by 50 orders of magnitude. What makes this exciting is that liquid deuterium or deuterium in some metals like palladium can have densities exceeding 1E22 atoms/cm3.

In March, 1989, a preprint of the same S.E. Jones et al. began to circulate describing the observation of RTF from a small electrolytic cell. The cell consisted of a palladium or titanium cathode, and a gold foil anode, immersed in heavy water (D2O). The water contained a witch's brew of metallic salts and nitric acid. They used a neutron spectrometer to analyze any nuclear reaction products. The d-d reaction goes like:

```
d + d -->> He3(0.8 MeV) + n(2.4 MeV)
      -->> H3(1.0 MeV)  + p(3.0 MeV)
```

with each channel equally likely. The upper reaction produces neutrons at 2.4 MeV which have a range of a meter in concrete and which can escape the cell (the other particles are absorbed within a mm of liquid). The Jones' paper clearly shows a neutron peak at 2.4 MeV which only appears with the cell operating. The authors spent considerable time trying to get a false reading from the spectrometer, but could not reproduce the peak in any manner except by operating the electrolytic cell near it. The result was quite convincing that they had observed RTF.

References :

(1) F. C. Frank, Nature, vol. 160, 525 (1947).
(2) L. W. Alvarez et al., Phys. Ref., vol. 105, 1127 (1957).
(3) C. DeW. Van Siclen and S. E. Jones, J. Phys. G: Nucl. Phys., vol. 12, 213 (1986).
(4) "Observation of Cold Nuclear Fusion in Condensed Matter",

S. E. Jones, E. P. Palmer, J. B. Czirr, D. L. Decker, G. L. Jensen. J Thorne, S. F. Taylor and J. Rafelski, preprint from Brignam Young University dated 3/23/1989.

J. F. Ziegler, IBM-Research, Yorktown, NY

----- FUSION FORUM appended at 20:03:04 on 89/03/30 GMT (by ZIEGLER at YKTVMV)
Re: RADIATION SAFETY in conducting Fusion Experiments

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*******************************************
***                                     ***
***         RADIATION   SAFETY          ***
***       for FUSION EXPERIMENTS        ***
***                                     ***
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SUMMARY : Fusion experiments such as reported from Utah MUST be conducted with some shielding. Six inches of glass, water or plastic should surround the experiment (approx 0.05 factor dose reduction). This will NOT be enough if your yield is more than one fusion per second. You MUST have neutron radiation detection equipment with efficiencies above 10% to be safe. No information is known about other forms of radiation (such as gamma rays) from the experiment. It is clear that this experiment may get very dangerous. -- J. F. Ziegler

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The following information is provided by the IBM-Research (Yorktown) Radiation Safety Officer, Jeff Leavey :

MONITORING REQUIREMENTS

All work with ionizing radiation should be done under the watch of a Radiation Safety Officer and monitored with the appropriate instruments. For fusion experiments, the minimum monitoring requirements are:

1. A calibrated neutron meter (with an integrating feature if possible)
2. An ion chamber (NOT a GM meter) for gamma dose monitoring. GM meters do not measure dose no matter what the dial says (unless calibrated for a specific energy(ies).
3. Personnel radiation monitoring badges for gamma and neutron.

INTRODUCTION

There are 4 particles produced in a d-d reaction. The reaction is :

$$\begin{aligned} d + d &\text{ -->> } He3(0.8\ \text{MeV}) + n(2.4\ \text{MeV}) + \text{gammas} \\ &\text{ -->> } H3(1.0\ \text{MeV}) + p(3.0\ \text{MeV}) + \text{gammas} \end{aligned}$$

with each channel equally likely. The upper reaction which produces energetic neutrons is the one of most concern. The proton, He3, and H3 being charged are easily stopped in water. The H3 (tritium) is radioactive and can easily become airborne via natural evaporation or heating the water; the health effect will depend on the quantity present. The t-d reaction, which produces 14 MeV neutrons must also be considered.

Out of all the public data available now, little is known about the reaction rates or just how much improvement is possible. Because of this, the information below is provided on a "per something" basis; it can be scaled up or down as needed. Increased efficiency gains might produce radiation levels above legal limits or that can impact your health.

NEUTRON DOSES

For 2.4 MeV neutrons, the current flux to dose conversion is 20 n/cm2-sec = 2.5 mrem/hr (ignoring thermalization for now) (NRC - 10CFR20.4). The Brigham Young experiment reportedly produced about 1,000 neutrons every 45 minutes. Not knowing what the detector area was, we assume a typical 2 inch diameter detector. The flux at the detector becomes 1.8E-2 n/cm2-sec and the dose rate becomes 2E-3 mrem/hr. Alternatively, if we assume the source rate is 1000 n/45 min and is a point source, then 6 inches from the experiment the B-Y neutron dose rate was approximately 2E-5 mrem/hr. If you get lucky and increase the fusion yield, the dose rate also scales up. Monitoring is VERY important.

(If we assume 2.4 MeV neutrons are released in D2O, then an average of 25 collisions with D are required to thermalize 0.025 eV. The slowing down length sqrt of the Fermi Age gives the average distance in D2O from approx 2.5 MeV to 0.025 ev and is 11 cm. This means 2.4 MeV neutrons are thermalized after 11 cm of D2O. If the experiment has less than 11 cm of D2O shielding then it is prudent to assume the neutrons are full energy. If the neutrons are thermalized, then the flux to dose conversion is 670 n/cm2-sec = 2.5 mrem/hr.) (Glasstone and Sesonske, NUCLEAR REACTOR ENGINEERING, pg. 133, 147)

For 14 MeV neutrons, the flux to dose conversion is 10 n/cm2-sec = 2.5 mrem/hr (NRC 10CFR20.4). An average of 28 collisions with D are needed to thermalize these neutrons with a small increase in the distance to thermalization.

NEUTRON SHIELDING

Neutron shielding, like gamma shielding, can be treated as exponential attenuation: I/Io = exp -(ux) for ease of calculation. NCRP 38 (Protection Against Neutron Radiation) gives for 2 MeV neutrons in polyethylene or water (1 in. poly = 1.2 in. water) an attenuation coefficient of u = 0.45 per inch (including build-up and scatter).
In the equation, x = thickness of shield in inches.

The 14 MeV neutrons produced by the 1 MeV tritium going into the deuterated water electrolyte occur once for each 1/10,000 fusions, so they are not important unless the reaction rate increases several orders of magnitude above the current levels. For 14 MeV neutrons the attenuation coefficient u = 0.1 per cm (0.2 per in.). (Rad Health Handbook)

GAMMA DOSES

This is the wild card. Gammas are emitted to take care of any residual energy after the fusion process. The gamma energy and number of photons is varied and has to be measured to get general values. Gamma monitoring is NECESSARY.

DOSE LIMITS and BIOEFFECTS

The legal (NRC) dose limits are 5000 mrem/yr from all radiations to the whole body (head, eyes, chest, gonads, blood forming organs). There is a 3 month limit of 1250 mrem/qrt yr to ensure the yearly limit is not exceeded too quickly. The public, or unrestricted area dose is 500 mrem/yr. While the IBM Industrial Hygiene Manual uses the NRC limits, it is prudent to utilize 10% of the legal limits.

No acute health effects are generally seen below 50 rem. Above this value changes in blood counts can be seen. At about 100 rem nausea starts and blood changes are seen. At 200+ rem the bone marrow is suppressed and the body's ability to fight secondary infections is decreased. 400-600 rem is fatal to about 50% of those exposed. At higher doses the central nervous system and GI tract are affected.

Long term risks, namely cancer, are increased with dose and duration of exposure. There are risk values available but individual lifestyle variations makes it difficult to assess risk at low doses.

Contact your site Radiation Safety Officer for additional information and guidance.

J. A. Leavey, Certified Health Physicist, IH&S (TL 862-3950)
J. F. Ziegler, Radiation Sciences Dept. (TL 862-2225)
IBM-Research, Yorktown

----- FUSION FORUM appended at 20:28:26 on 89/03/30 GMT (by ZIEGLER at YKTVMV)

Re: Dangers in Fusion Experiments

I would like to emphasize the basic message of the long appends above about safety. You can not set up the experiment and just look for HEAT to see if you have fusion. This experiment, if it works as reported, is too dangerous to go plunging into without expert help. Nuclear Radiation can not be felt until it is too late. I keep hearing about groups setting up the experiment with only calorimeters as diagnostic tools. That is very very dangerous. And remember, "Geiger Counters" and other simple radiation monitors will not detect ANY of the fusion particles.

J. F. Ziegler, IBM - Research

----- FUSION FORUM appended at 14:06:46 on 89/03/29 GMT (by ZIEGLER at YKTVMV)

Subject : The Witch's Brew of Brigham Young University

The observation of Cold Nuclear Fusion (CNF) by scientists at Brigham Young University invoved a complex Electrolyte. Any comments and what may be most important in this concoction would be most helpful. Their recipe was :

"The electrolyte is a mixture of 160g deuterium oxide (D2O) plus various metal salts in 0.2 g amounts each :

FeSO4 in 7 H2O
NiCl2 in 6 H2O
PdCl2
CaCo3
Li2SO4 in H2O
NaSO4 in 10 H2O
CaH4 (PO4)2 in H2O
TiOSO4 x H2SO4 in 8 H2O

and a very small amount of AuCN."

" (Our evidence indicates the importance of co-deposition of deuterons and metal ions at the negative electrode.) The pH is adjusted to pH <= 3 with HNO3. Titanium and palladium, initially selected because of their large capacities for holding hydrogen and forming hydrides, were found to be effective negative electrodes " .... in producing CNF.

J. F. Ziegler, IBM - Research, Yorktown

----- FUSION FORUM appended at 16:10:50 on 89/03/29 GMT (by MARWICK at YKTVMV)
Subject: Hydrogen site in Palladium
Ref: Append at 15:11:24 on 89/03/29 GMT (by CHALLENE at FSHVMFK1)

The site occupied by Deuterium in Palladium is the ocatahedral interstitial site. These sites in the fcc Pd lattice lie in the middle of the cube edges. Since the length of a cube edge is 3.88 A in Pd, these octahedral sites are 0.388/sqrt(2) = 2.74 A apart. So, Deuterium in these sites isn't very likely to fuse.

If (a big if) some tetrahedral intersititial sites are also occupied, then the d-d spacing in neighboring tetrahedral and octahedral interstitial sites would be 1.6 A, which is obviously in the right direction, but still much larger than the D-D spacing in a molecule of D2 gas: 0.74164 A.

But this just says that if the RTF effect is real, then it isn't due to deuterium on regular interstitial sites.

Alan Marwick

----- FUSION FORUM appended at 17:22:15 on 89/03/29 GMT (by SOREFF at FSHVMFK1)
Subject: Atoms in liquid deuterium =
Ref: Append by CHALLENE at FSHVMFK1 on 89/03/29 at 15:11:24 GMT.
The reason that you can have deuterium atoms at .7 angstroms from each other in liquid deuterium, yet have an average density of .01 deuterons/cubic angstrom, is that deuterium molecules are diatomic, with a short intramolecular internuclear distance, but the INTERmolecular distance in the liquid is much longer, set by the Van Der Waals forces between the molecules.

-Jeffrey Soreff

----- FUSION FORUM appended at 00:51:11 on 89/03/30 GMT (by MMFARROW at ALMVMC)
Subject: Room Temperature Fusion experiments
Ref: Append at 18:11:55 on 89/03/29 GMT (by MMFARROW at ALMVMC)

I am a member of a small group here at Almaden Research that is trying to reproduce the Pons/Fleischmann experiment (from news reports!). I will give a very brief summary of our (unsuccessful) efforts to date.

Since we are aware that 2 MeV neutrons will be hard to detect, we are using a crude 'calorimeter' experiment. Our initial attempt used a Pt mesh anode and a palladium slug (0.25" diameter X 0.5" long) cathode. Using 99.8% D2O, we made a 0.5 M sodium sulfate electrolyte solution which we then electrolysed for >18 hrs at about 14 volts, and 1 amp. No thermal excursions were detected. Thermal capacity and conductance estimates (made by changing the power lost in the cell from IR losses) indicated a sensitivity to a few watts change in heat flow (which would arise from 'fusion').

Dick Peekema suggested using crystal violet to inhibit the recombination of atomic deuterium. This did increase the overpotential for D2 evolution. No thermal excursions were detected.

Today's rumor was that the electrolyte should be 0.1 LiOD (deuterated lithium hydroxide), which we prepared from litium/D2O. The electrolysis cell was modified to reduce the volume and increase the thermal sensitivity. The cell is immersed in a 4 l bath with sodium tetraborate (sat'd). After 3 hours of electrolysis at 5 V/1 A, no thermal excursions.

Almaden Group

Please contact Joe Gordon Gordon at ALMVMC

----- FUSION FORUM appended at 12:09:47 on 89/03/30 GMT (by MIKCLRK at HVTVM2)
Subject: Room Temperature Fusion experiments

A few thoughts - I don't have the reference materials or the maths to check them out my-self, but they might suggest something to someone.

Is the duterium acting like a metal when it's deposited onto the palladium ? - this should give rise to a coating of 'metallic' duterium on the surface of the cathode.

In normal metals the outer electrons of the metal are disassociated from their parent atoms into an electron cloud that sits between all the atoms - the atomic nuclei are still surrounded by a (complete) shell of electrons so they stay a respectable distance apart. If, somehow, the combination of the palladium, the electric current and what ever impurities are present caused the duterium to bond in this manner, similar to normal metals being deposited by electrolysis. The result would be unusual as the duterium atoms have only one electron to lose and the ion that is left consists only of the much smaller nucleus - if these were to pack into a lattice like normal metal ions the spacings could be much less.

Does anyone know what happens to the probabilities of fusion occurring when we are dealing with duterium ions, rather than atoms ?

Detecting fusion - Run the experiment in a sealed chamber and check periodically to see if any more helium has turned up ?

Mik Clarke

----- FUSION FORUM appended at 13:59:47 on 89/03/30 GMT (by RLG2 at YKTVMV) ---
As stated by Jim Ziegler (above), the D + D reaction goes in about 50% of the cases to N + He-3 and the other half to P + H-3 (tritium). Anyone who sees a "One-watt" thermal excursion as a way of detecting cold fusion is likely not to live to enjoy the fame of the discovery, and that is probably the most suspect feature of the Pons-Fleischmann claim.

One watt of power is about 0.3 watts in neutrons. The lethal dose of whole- body radiation is about 400 rem (Roentgen-equivalent-man), with one roentgen about 100 ergs of energy deposited per gram of tissue.

At a distance r from the source, with neutrons scattering from the protons in tissue with a mean-free-path of about 10 cm or 10 g/sq cm, the energy deposited is

   (0.3 x 10**7 ergs/sec)/ (4 pi r**2) per sq cm.   At r = 100 cm (one
meter) (energy per sec)   (area).

this is about 30 ergs/sq cm and per sec. This is then about 3 ergs / gm-sec or about 10,000 ergs/gm-hr. If neutrons were only as bad as gamma rays for the health/life of mammals, this would be 100 roentgens/hr or 100 rem/hr.

But neutrons have a "relative biological effectiveness" of 10, so 10,000 ergs/g m-hr is 1000 rem/hr.

Fifteen minutes of proximity to a one-watt fusion source will provide a lethal dose of radiation.

PLEASE DO NOT TRY TO OBSERVE FUSION BY ITS HEAT EFFECTS. Neutron detection is a billion times more sensitive.

Dick Garwin

----- FUSION FORUM appended at 16:30:06 on 89/03/30 GMT (by VOYAGER at KGNVMC)
Subject: Dick Garwin's Append
Ref: Append at 15:02:38 on 89/03/30 GMT (by MPREDKO at TORVMFG1)

The number of neutrons produced per fission is dependent on the nucleus being split. Most of todays nuclear reactors use an isotope of Uranium that undergoes what is known as slow fission, and produces approximately 2.3 neutrons per fission.

I thought that when considering the amount of energy passing a given area, it should be treated as a point source, unless sufficiently close. Thus, the energy at a given distance from the source varies with the inverse square of the distance. (ie doubling the distance reduces the energy by 1/4). This also applies to radiation of various forms. To compute the amount of energy per area, I would have thought that it was necessary to divide by the area of a sphere with radius r (4/3 pi r**3 ?).

I also feel that the amount of radiation calculated (1000REM/hr) for .3 watt is a bit high. Hydrogenated material is generally the best shielding for neutron radiation. I believe the half thickness for this is about a foot. It would require several feet of water to shield against this radiation. I've worked in close proximity (20-30 feet) of several operating fission reactors producing Megawatts of power, and have never seen that much shielding. Am I missing something?

Rob Maiolini (ex-nuke)

----- FUSION FORUM appended at 16:57:24 on 89/03/30 GMT (by MARWICK at YKTVMV)
Subject: Hydrogen site in Palladium
Ref: Append at 18:03:14 on 89/03/29 GMT (by EDWARDS at LEXVMC)

Jonathon - I don't know if two deuterons can occupy the same interstitial site in Pd. I do know that in silicon it is believed that hydrogen molecules can form in interstitial sites, but those sites are much larger and more open than those in Pd. Also, the electron density in those sites is much smaller. If anyone has a reference or definite knowledge on this point in Pd, I'd like to hear about it.

Alan Marwick

----- FUSION FORUM appended at 17:48:26 on 89/03/30 GMT (by HORKANS at YKTVMV)
Re: References for electrosorbed H in Pd

I am appending some literature references from my file on hydrogen in Pd. These are electrochemical reference, and I make no claims for completeness, because I haven't tried to follow this subject very closely.

A caution. Electrochemists generally study the alpha phase. I doubt that this is the relevant phase when there are large amounts of absorbed hydrogen. In that case, I believe that we are dealing with the beta phase. There is a structure change upon the phase transition, leading to gross deformation of the electrode.

Anyway, here, for what it is worth, is an incomplete reference list pertinent to (probably) the wrong phase of Pd-H. Maybe it will lead into more relevant references.

03/30/89 12:31:26 HORKANS
L. Stoicoviciu and R. V. Bucur, J. Electroanal. Chem., 21, 307 (1969).
"THE ROLE OF THE METAL-SOLUTION INTERFACE ON THE MECHANISM OF THE DISSOLUTION OF HYDROGEN IN Pd"
R. V. Bucur and L. Stoicovici, J. Electroanal. Chem., 25, 342 (1970).
"HYDROGEN SOLUBILITY IN ELECTROLYTICALLY DEPOSITED THIN FILMS OF Pd"
M. W. Breiter, J. Electroanal. Chem., 81, 275 (1977).
"DISSOLUTION AND ADSORPTION OF HYDROGEN AT SMOOTH Pd WIRES AT POTENTIALS AT THE ALPHA PHASE IN H2SO4 SOLUTION"
C. T. Campbell, D. C. Foyt, and J. M. White, J. Phys. Chem., 81, 491 (1977).
"OXYGEN PENETRATION INTO THE BULK OF Pd"
M. W. Breiter, J. Electroanal. Chem., 90, 425 (1978).
"DISSOLUTION AND ADSORPTION OF HYDROGEN AT SMOOTH Pd WIRES AT POTENTIALS IN THE ALPHA PHASE. INFLUENCE OF ELECTROLYTE"
R. V. Bucur and I. Covaci, Electrochim. Acta., 24, 1213 (1979)
"GALVANOSTATIC DESORPTION OF HYDROGEN FROM Pd LAYERS. I. THEORY"
T. Maoka and M. Enyo, Surface Tech., 8, 441 (1979).
"OVERPOTENTIAL DECAY TRANSIENTS AND THE REACTION MECHANISM OF THE Pd-H2 ELECTRODE"
J. Horkans, J. Electroanal. Chem., 106, 245 (1980).
"FILM THICKNESS EFFECTS ON HYDROGEN SORPTION AT Pd ELECTRODES"
N. A. Zakarina, N. F. Toktabaeva, G. D. Zakumbaeva, and R. S. Miner, Sov. Electrochem., 16, 664 (1980).
"EFFECT OF THE DEGREE OF DISPERSION ON HYDROGEN SORPTION BY Pd CATALYSTS"
R. V. Bucur and F. Bota, Electrochim. Acta, 26, 1653 (1981).
"EFFECT OF ANION ON THE TRANSFER EQUILIBRIUM AT THE Pd-H/ELECTROLYTE INTERFACE"
R. V. Bucur and F. Bota, Electrochim. Acta, 27, 521 (1982).
"GALVANOSTATIC DESORPTION OF HYDROGEN FROM Pd LAYERS. II. THE TRANSFER PROCESS"
A. Kufudakis, J. Cermak, and F. A. Lewis, Surface Tech., 16, 57 (1982).
"REVERSIBLE AND IRREVERSIBLE DIFFUSION--ELASTIC DEFORMATION EFFECTS

RESULTING FROM ABSORPTION AND DESORPTION OF HYDROGEN BY Pd"

R. V. Bucur and F. Bota, Electrochim. Acta, 28, 1373 (1983).
"GALVANOSTATIC DESORPTION OF HYDROGEN FROM Pd LAYERS.
III. THE ANODIC VOLMER REACTION"

R. V. Bucur and F. Bota, Electrochim. Acta, 29, 103 (1984).
"TRANSFER EQUILIBRIUM IN THE SURFACE LAYER OF A (Pd-H)-ELECTRODE
WITH LOW HYDROGEN CONTENT"

R. V. Bucur and F. Bota, Electrochim. Acta, 29, 1283 (1984).
"INFLUENCE OF INTERFACE PROPERTIES ON GALVANOSTATIC DESORPTION
OF HYDROGEN FROM FINITE Pd-H ELECTRODES"

F. A. Lewis and S. G. McKee, Surface Tech., 24, 355 (1985).
"STRUCTURE OF HYDRIDED Pd AT INTERFACES"

R. V. Bucur, Electrochim. Acta, 31, 385 (1986).
"DIFFUSION AND EQUILIBRIUM PROPERTIES OF HYDROGEN IN Pd"

R. V. Bucur, Surf. Coat. Tech., 28, 413 (1986).
"INFLUENCE OF THE SUBSURFACE LAYER ON THE MEASURMENTS OF THE
DIFFUSION COEFFICIENT IN POLYCRYSTALLINE Pd"

E. S. Carnell and S. P. Wach., Surf. Soat. Tech., 28, 339 (1986).
"TRANSPORT COEFFICIENTS AND ENERGETICS OF HYDROGEN IN Pd"

J. Horkans, J. Electroanal. Chem., 209, 371 (1986).
"HYDROGEN REGION OF THE CYCLIC VOLTAMMETRY OF Pd: EFFECT OF pH AND
ANION"

M. R. Hawkesworth and J. P. G. Farr, J. Electroanal. Chem.,
119, 49 (1981).
"COLD NEUTRON RADIOGRAPHY OF HYDROGENATED PALLADIUM"

Jean Horkans

----- FUSION FORUM appended at 18:27:49 on 89/03/30 GMT (by LEAVEY at YKTVMV) -
..... FUSION FORUM modified at 15:31:52 on 89/03/31 GMT (by LEAVEY at YKTVMV) .
Re: Dose Estimate

I too looked at what I would expect from 0.3 watt of neutrons. Using 1 watt = 6.2E12 MeV/sec and assuming the neutrons thermalize in tissue (2.4 MeV --> 0.025 eV), all their energy is given up to tissue. This equates to 7.8E11 n/sec at 2.4 MeV/n. Using a point source at 100 cm radius, the flux is 6E6 n/cm2-sec.

The neutron dose rate can be estimated from:

```
                                               █‾
        (n/cm2-sec) (MeV/n) (1.6E-13 J/MeV) /_ (N sig f)
    D = -------------------------------------------------
                1 J/kg-Gy
```

where:
n/cm2-sec = 6E6
MeV/n = 2.4
N = atoms per kg of the elements in tissue
sig = scatter x-section for the tissue element
f = average neutron energy transferred per collision for each tissue element (function of tissue element only, not neutron energy).

```
                 2M
           f = --------    M = atomic number
               (M+1)**2
```

Tissue can be considered to be made of O, C, H, N, Na, Cl. Sum the product of N, sig, and f for all six.

For 2.4 MeV neutrons, the sum of (N sig F) over the 6 elements is 76 cm2/kg. Plugging this into the equation gives the absorbed dose in grays (= 100 rad):

D = 1.8 E-4 Gy/sec = 1.8 E-2 rad/sec = 650 rem/hr at 1 meter.

The quality factor for fast neutrons is 10 rem per rad. Going in to 6 inches raises the dose rate increases to about 27900 rem/hr. These values reasonably agree with Dick Garwin's enough to illustrate that these people should be real sick by now.

(Ref: H. Cember, HEALTH PHYSICS and NCRP 38, PROTECTION AGAINST NEUTRON RADIATION)

Jeff Leavey, CHP TL 862-3950 Yorktown LEAVEY at YKTVMZ

----- FUSION FORUM appended at 09:17:09 on 89/03/31 GMT (by MIKCLRK at HVTVM2)
Subject: Room Temperature Fusion experiments
Ref: Append at 12:09:47 on 89/03/30 GMT (by MIKCLRK at HVTVM2)

More thoughts (if someone can tell me this is wrong I'll stop wasting our time)...

Does the palladium have to be saturated with duterium before fusion will occur ? Did the origonal group use the same electrodes through out their experiments ?

If the electron on the duterium is being raised to sufficient energy to join the conduction band of the palladium (thus become disassociated from it's nuclius) the resulting nucli would have very little trouble diffusing into the palladium (it's about half an alpha particle and will consider metals to be mainly empty space). Any "surface" deposits of duterium will diappear into the palladium before a duterium lattice can begin to form, meaning that you'll only get the very close duterium nucli after the palladium is close to being saturated with duterium.

Would a fusion reaction disrupt the lattice arrangement near it ? This may have a dampening effect, greatly reducing the probability further fussion reactions in the area - a run-away chain reaction might account for the report of the reaction vessel melting a hole in the floor.

If the reaction is occuring amongst very densely packed duterium nucli the average flight path of a neutron through it would be greatly reduced, possably accounting for the lack of observed radiation (and the continued survival of the experimenters :-).

Mik Clarke

----- FUSION FORUM appended at 15:13:12 on 89/03/31 GMT (by LEAVEY at YKTVMV) -
Re: Tritium detection

There's been discussion as to fusion products that should be present after the reaction (e.g. neutrons, gammas, etc), including tritium. For those who may not be familiar with radioactive materials, a standard method for counting H-3 is liquid scintillation counting. It is a very sensitive method with modern l-s counters and can provide quantitative information like disintegrations/time-volume. I throw this out to add to the arsenal of tools available to verify RTF.

Jeff Leavey TL 862-3950 LEAVEY at YKTVMZ

----- FUSION FORUM appended at 16:00:37 on 89/03/31 GMT (by MCINNIS at AUSVM9)
Subject: Dangers of RTF

Since we don't really understand yet what's happening (a true statement whether or not fusion is actually occurring), couldn't there be an even less subtle hazard from RTF experiments? What if you get a fair amount of duterium in the palladium and it all decides to fuse in a microsecond? What if it turns out that you too can build an H-bomb in your basement with $50 worth of chemicals?

Could there be some form of positive feedback or chain reaction in RTF? There was a report of one small runaway reaction that melted the experimental apparatus.

We're all talking about the great potential for good of this. What if it has potential for evil?

Mickey McInnis (MCINNIS AT AUSVM9) B5 678/6779 Austin, Texas

(I can't claim to be an expert, so I'll shut up now. I think this is a fair safety question for an amateur to ask in an "expert" forum, though.)

----- FUSION FORUM appended at 16:09:56 on 89/03/31 GMT (by WHITEJM at CLTVM2)
Subject: Room Temperature Fusion experiments
Re: 89/03/31 append by MIKCLRK at HVTVM2
I would be concerned that disturbing the lattice might also INCREASE the possible of reactions -- quite the opposite of a dampening effect. Maybe this would explain the hole in the floor?

Jim White

----- FUSION FORUM appended at 23:28:41 on 89/03/31 GMT (by MMFARROW at ALMVMC)
Subject: Pons lecture at the University of Utah
Ref: Append at 20:38:40 on 89/03/31 GMT (by PDC at SJEVM5)

I have just gotten of the phone with E.M. Eyring at the University of Utah. Stan Pons has just finished giving a lecture at Utah. No pictures were allowed during the lecture. Bottom line: 0.1 M LiOD electrolyze for "weeks" to prepare what is speculated to be the beta phase. Ted Eyring is convinced that Pons has something. Much resistance from the physics community. Pons will be giving a lecture at Indiana U, Bloomington early next week. His host is Gary Hieftje. (Hieftje's phone # is 812-335-2189, Departmental # 812-335-9043).

Second call to Ted: No "catalyst" during fusion, just 0.1 M LiOD. No info (very bad acoustics) on neutron detection method. Pons did detect 2.3 MeV gamma rays, tritium, neutrons. Neutrons appeared proportional to tritium. NOTE: the 4x yield >>>includes<<< all of the energy expended during the charging period - no apparent need to boost the current to achieve fusion.

Electrode geometry is critical - no sharp edges, don't use sintered or "compacted" Pd pellets. Do not expose the charged Pd to steep concentration gradients. Don't turn off the cell, slowly 'wind it down'. Otherwise - boom. He used borrowed Johnson Mathey (sp?) cast or machined shapes. Fusion detected with a simple calorimeter - thermocouple?

Story: Pons' son Joey was at U of U doing the experiments. HAD THE

INFAMOUS MELTDOWN. Joey left for No. Carolina (where the Pons family lives). "Mormon" graduate student took over (Marvin). Unclear whether Dad was more worried about his posterity (and 2.2 MeV gammas) or the unreliability of son.

Mike Farrow

----- FUSION FORUM appended at 23:08:12 on 89/04/01 GMT (by ZIEGLER at YKTVMV)
Re: Brigham Young Experiment

On March 31, Steven Jones of Brigham Young University gave a colloquium to an overflow audience at Columbia University in New York City. This was the first public talk on his "Cold Nuclear Fusion" experiments.

The talk followed closely to his paper, with only a few additions. These are noted below :

(1) He NEVER observed heat. He estimated his "heat" to be about 10E-13 watts.

(2) His maximum neutron rate was about 0.6/second. This has been corrected for his detector efficiency. Since the neutrons only appear in half the reactions for "normal" d-d nuclear fusion, that means his real fusion rate was 1.2/second. This best-run corresponds to run #6 in his paper.

(3) The strange "witch's brew" for an electrolyte arose from an analysis of the lava from a volcano eruption in Hawaii. It had been noted that this lava had an unusual He3/He4 ratio. Jones felt that somehow this meant that some unusual chemical reaction was going on which led to nuclear fusion and hence the excess 3He. He also said the brew came from an analysis of the Earth's crust. He seemed to have just added more salts as new ideas came to him over the last two years. He never tried to find which of the salts were important.
Before he used the mixture, he noted (under questioning) that the bottom of the electrolyte was "muddy". He decanted the mixture leaving the mud out of the electrolytic solution under test.

(4) He observed no change in neutron signal rate with variation of electrolyte current. Current ranged from 20 - 100 mA.

(5) Among the null tests he ran to confirm his neutron signal, were identical tests using water instead of heavy water. All other parts of the experiment were the same. He observed no neutrons with ordinary water in the electrolyte. He performed many other kinds of experiments, and always observed no neutrons except when the experiment was correctly set up.

(6) With new samples, neutrons increased for the first hour, then stabilized in rate, then the neutrons disappeared after about 8 hours when the cathode became coated with iron.

(7) His explanations for the cause of "Cold Nuclear Fusion" were vague. The only item which made reasonable sense was that the deuterons were getting within 0.4 A of each other, and tunnelling would account for the rest. This is came from his 1986 paper noted in the Append above on Historical Perspective and makes sense.

COMMENTS :

The talk covered work of the last two years. He showed notebooks dating back to 1987 with experiments showing neutrons. It is clear that this work has been proceeding at a leisurely pace, with about 16 experiments occurring in two years. They seem never to have tried to optimize much, especially the electrolyte.

His work is about 50% convincing. But with the simultaneous claims by Pons and Fleischman, both scientists of considerable reputation, one must think that there is even stronger probability that they have discovered cold nuclear fusion. In Pons' talk, noted above, they also state that they see neutrons. Free energetic neutrons do not come from any chemical reaction that only contains light elements and palladium or titanium. That Jones found both titanium and palladium equally effective in making cold fusion, makes one think that cathode contamination with uranium, or other alpha-emitters, is an unlikely cause of his effect. But his casualness in material handling does not rule it out.

J. F. Ziegler, IBM-Research, Yorktown

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Date: 1 Apr 89 14:10:25-PST (Sat)
From: Physics-Request@unix.SRI.COM
To: Physics@unix.SRI.COM
Subject: Cold Fusion preprint from Fleischmann and Pons
Reply-To: Physics@SRI-Unix.arpa
Reply-To: jhh@pupthy.PRINCETON.EDU (Jim Horne)

Date: 31 Mar 89 19:25:18 GMT
From: jhh@pupthy.PRINCETON.EDU (Jim Horne)
Article-I.D.: <7514@phoenix.Princeton.EDU>

The preprint of the Fleischmann and Pons paper is now being distributed. The paper is called

"Electrochemically Induced Nuclear Fusion of Deuterium"

by
Martin Fleischmann, Department of Chemistry
The University, Southampton, Hants. SO9 5NH, England
and
Stanley Pons*, Department of Chemistry
University of Utah, Salt Lake City, UT 84112 USA

*to whom correspondence should be addressed.

It was submitted to the Journal of Electroanalytic Chemistry on March 11, 1989; in final form March 20, 1989.

I'm not going to type in the whole thing, but a brief summary follows. The basic experimental setup is described as "D+ was compressed galvanostatically into sheet, rod and cubic samples of Pd from 0.1 M LiOD in 99.5% D2O + 0.5% H2O solutions." They don't really describe things in much more detail.

They ran four types of experiments.
1) "Calorimetric measurements of heat balances at low current densities"

2) "Calorimetric measurements at higher current densities"
3) "The spectrum of gamma-rays ... due to the (n,gamma) reaction"
4) "The rate of generation/accumulation of tritum"

The results from 1) and 2) [in my opinion the most questionable ones] are "enthalpy generation can exceed 10 watts/cm¬3 of the palladium electrode; this is maintained for experiment times in excess of 120 hours during which typically heat in excess of 4 MJ/cm¬3 of electrode volume was liberated. It is inconceivable that this could be due to anything but nuclear processes."

It is not very clear to me how they made sure they had subtracted all possible energy produced in chemical reactions. An obvious test would be to run the experiment with pure H2O and compare the heating rates.

The result of 3) is the most impressive. They put a water bath nearby to soak up the neutrons produced, and convert them into gamma-rays. Figure 1A shows a graph of the gamma-ray spectrum, which has a peak of about 21000 counts per channel at an energy of about 2.21 MeV. The background level is 400 counts per channel. They is no way these photons can be produced in a chemical reaction. From the intensity of the photon flux, they estimate the D+D -> He3+n to be 4*10¬4/sec for a 0.4x10cm rod.

For experiment 4) they measure the tritium production rate, and get a rate of 1-2*10¬4 atoms/sec.

The reaction rates given by 3) and 4) are much too small to account for the energy production in 1) and 2), by a factor of about 10¬9. They conclude that the He3 and T reactions "are only a small part of the overall reaction scheme and that other nuclear processes must be involved."

Thus there still seems to be a problem with the total heat production. Their evidence for fusion seems clear, but the total rate seems rather uncertain. I would be much more skeptical if I hadn't also read the BYU preprint from yesterday. There are at least three groups at Princeton trying to reproduce the results, none of which have seen anything yet.

In a week or two, we should know more. Remember, kids, don't try this at home unless you want your baby brother to have three arms.

Jim Horne
jhh@pupthy.Princeton.EDU

----- FUSION FORUM appended at 18:53:59 on 89/04/02 GMT (by ENGER at PKEDVM9) -
Subject: 2nd Group Reports on █Cold Fusion'
Ref: Append at 18:32:55 on 89/04/02 GMT (by MJK at RALVM22)
in NEWSCLIP FORUM

----- FUSION FORUM appended at 04:57:23 on 89/04/03 GMT (by RMILLER at YKTVMV)
I would like to add some information to the forum based on experience I had some 15 years ago electrolytically loading H and D into Pd. First a caution: I will be describing a chemical procedure that may sound safe, and at the time we believed it was, and indeed it may still be, but at the time no thoughts of inducing fusion ever entered our heads. As far as we know, we never created any fusion (and you'd think if we had done so, we'd notice, right? Well, maybe not based on Jones's indication of 200 neutrons/hr.) If you intend to try the procedure, with or without some modifications of your own, and you think that you have ANY CHANCE at all of creating fusion (and face it, now, why are you trying it if

you don't have some hope of it "working"?), please do so ONLY including observing all the precautions about safety that Jim Ziegler appended above.

By electrolytically loading H or D into Pd in a low temperature bath (dry ice/acetone at ca. -78 deg. C) from a acidified bath of methanol, we were able to achieve H or D to Pd atomic ratios up to nearly 1-to-1 (More specifically, 0.98 H/Pd ratio, and 0.96 D/Pd ratio. These are separate samples, not both H and D in the same sample. It is not, however, isolated instances; we had 4 such samples of the Pd-H at 0.98 and 5 of Pd-D at 0.96) The procedure is essentially the same as that used by Skoskiewicz (1972) and Harper, Hammond, and Geballe (1974). A Pt anode and a Pd foil cathode are immersed in a cell containing (for the hydride) 1 part conc. HCl to 9 parts CH3OH, or (for the deuteride) 1 part conc. DCl (in D2O) to 9 parts CH3OD (we felt it not necessary to use CD3OD). The cell was surrounded with a dry ice/acetone bath and blanketed with dry N2 gas (thereby avoiding condensation). Charging times of 8 min. to 3 hr. were used, using a voltage of 20 V. to achieve a current density of about 100 mA/cm2 for our samples of 1.5 cm2 surface area (foils, 25 mm by 3 mm). In some cases, a small amount of thiourea was used as H2 recombination inhibitor, but in general we felt it had little if any effect. The samples have to be maintained cold (and were generally stored in liquid N2) in order that they would retain the hydrogen or deuterium. For further info, see Miller and Satterthwaite (Phys Rev Lett 1975 and the references in that paper).

We saw no indication of a fusion reaction occurring in our PdD samples (nor any adverse health effects), so it would appear that, if the recent results of either the Utah or BYU groups are correct, then either (a) they achieved higher than 1-to-1 ratios of D to Pd in their samples (causing occupancy of other interstitials, perhaps tetrahedral, or perhaps dual occupancy of a single interstitial), or else (b) something more than simply a high concentration of D in the Pd lattice is required before the conditions become favorable for fusion. There is another possibility also, and that is that the conditions for fusion are due to deuterium at the surface or close subsurface, and what is happening in the bulk is not relevant.

As Alan Marwick has indicated, above, the H or D atom occupies an octahedral interstitial site in the Pd lattice. Since Pd is a face-centered cubic material, the set of octahedral interstitial sites is another fcc sublattice, displaced by 1/2 the unit cell. (You can imagine the displacement to be in the X, Y or Z direction. You get the same result. Or displace in all three to body-center.) In stoichiometric PdH or PdD, the crystal structure becomes that of NaCl (all Na atoms on one fcc sublattice, all Cl atoms on the other fcc sublattice), so there is an expansion of the lattice constant, but no major restructuring of the relative position of the Pd atoms to accomodate the H or D. Except for differences in zero-point motion, the deuterium atoms are as far from their D neighbors as the Pd atoms are from their Pd neighbors.

Responding to Mik Clarke's append of 3/30/89 on whether the H or D acts like a metal in the lattice, there is evidence that the electrons from the H or D atoms play three roles as they enter into the electron energy bands of the material. The evidence comes from photoemission work performed by Eastman, Cashion, and Switendick (PRL, 1971; yes, that's our Dean Eastman). There occur some new levels about 5 eV below the Fermi level, that are associated with Pd-H bonding (hybridized states); some of the electrons enter the unfilled 4d states of Pd (the 0.36 d-band holes); and the rest begin filling the 5sp band by lowering it below the Fermi level. The material remains metallic. According to the E,C, and S. article, based on results of augmented plane wave calculations, the bonding states "are hybridized bonding states with greater than 0.6 electron of 1s character inside the hydrogen APW sphere (radius = 0.704 A). This charge is larger than the 0.5 electron inside the same size sphere for the hydrogen atom. Thus the proton is well screened in PdH and has about the same negative charge as it does in neutral hydrogen." Their conclusion seems to be that it is misleading to think of the H or D playing the role of an isolated proton (or deuteron) in PdH or PdD; that is, be wary of thinking of the deuteron as an ion, at least when considering the expected state of deuterium occuping the octahedral interstitials in the bulk material.

Pons has recently made mention of the Beta phase. A small amount (few atomic percent, depends on temperature) of H or D is "soluble" in Pd, and the H or D randomly occupies octahedral sites. This is referred to as the alpha phase. More H or D can enter the Pd lattice, but when it does, clustering of the H or D occurs, where clustering means that if one octa. site is occupied, the others nearby are likely to be occupied also. (Clustering does not mean two atoms on the same octa. site.) This is the beta phase, and at room temp and below, the minimum atomic ratio in the beta phase is about 0.63. The relative amounts of alpha and beta phase in the sample have to match the "lever rule" so that the bulk concentration of H to Pd is satisfied. As more H or D is added, more of the sample becomes beta phase, until the whole sample is beta phase at about 0.63, then still additional H or D addition results in the H/Pd ratio in the beta phase increasing toward 1.0. A coexistence region exists between the alpha and beta phases (for a sample with a bulk H/Pd ratio of 0.45 there are actually the presence of both the alpha and beta phases). Electrolytic charging will take samples through the coexistence region. Charging by high pressure gas at higher temperatures can avoid the coexistence region by going over the top of it. Critical pressure and temperature for Pd-D is 34 atm. and 550 K., according to Brodowski and Poeschel (1965). Above that, there is no miscibility gap. For Pd-H, the values are 19.9 atm, 567 K.

For those who are really getting into it, there is a resistivity anomaly that occurs near 55K, that has been associated with an ordering on the H or D sublattice (superlattice formation). I believe neutron diffraction demonstrated 1 plane vacant of D atoms for every 4 filled planes, in a direction that I believe was the 4,2,0 direction. As far as I know, this is not germane to the fusion question at all, just a curious piece of info.

Now that LiOD has been mentioned (see Almaden's appends of 3/30/89 and 3/31/89) is there any chance that the Li can be entering into the Pd lattice, as well as D? -- especially with the lengthy charging times? I recall that there was publication of work of B in Pd and of H in Pd-B alloy samples. I believe a high temp diffusion akin to that used to diffuse B into Si was used to put the B into the Pd. It seems I also recall that the miscibility gap of H in Pd-B had lower critical pressure and temperature than the case for H in Pd. Perhaps there are similar effects for Pd-Li and Pd-Li-D. Would Li-D fusion be an absurd speculation? Is all of this an absurd...?

Bob Miller, Yorktown

----- FUSION FORUM appended at 13:24:27 on 89/04/03 GMT (by RLG2 at YKTVMV) ---

Date: April 3, 1989
From: R.L. Garwin x2555 26-234 Yorktown Heights, NY
VNET: RLG2 at YKTVMV;

Sub.: Comment for IBM Fusion Forum on BYU palladium hydride explosions.

The telephone conversation of Mike Farrow with E.M. Eyring (appended above) provokes the hypothesis that the sudden energy releases observed in the Brigham Young University experiments are due to an autocatalytic electrochemical explosion of palladium hydride at high saturation levels.

The voltage applied to the electrolytic cell produces high effective concentrations of deuterons at the surface of the cathode-- "effective concentrations" in the sense of building interior concentrations of D to a level that could be achieved only by external pressure that is one atmosphere multiplied by a number whose natural logarithm is the ratio of the reversible overvoltage to the voltage corresponding to kT. For a singly charged species, the latter is 0.025 volts. Thus one can get to 10**5 atmospheres (1.5 million psi) (log = 5, and ln = 5 x 2.303 or 11.5) at a reversible overvoltage of 0.29 V. So what? The requirement of "no sharp corners ..., winding down the cell slowly" and so on would follow if one were observing the influence of internal pressure in the hydride on removal of the voltage.

While the cell is operating, deuterium is being forced into the lattice at an effective pressure proportional to the antilog of the applied voltage. If the relevant sites are uniformly populated, the internal pressure differences may be small compared with this effective pressure, but if the cell is suddenly stopped, the surface layers may be rapidly depleted, and the hydride may fragment.

In high explosive, the rate of decomposition of the chemical compound is a strong function of temperature (and above a certain temperature goes quite rapidly-- hence the characteristic brisance accompanying such a homogeneous release of internal energy. In contrast, the reaction between GRAINS found in gunpowder or other inhomogeneous material takes a longer time and can be used for propelling as well as for breaking.

Although the effective pressure in the hydride depends only on the overvoltage, the stored ENERGY depends also on the number of sites that might be occupied only at these high overvoltages.

So the explosive energy releases would come from a shattering of the hydride and a breaking of the container, followed by a reaction of the hydride with air.

No fusion energy is required.

This does NOT explain a continued net evolution of heat during electrolysis (greater than the product of voltage times current applied to the cell), IF such actually exists.

R.L. Garwin

RLG:jtml:093%FILE:040389FILE

----- FUSION FORUM appended at 14:31:45 on 89/04/03 GMT (by GILBT at YKTVMV) --
Subject: RTF

The following is the text of a handout which was given to most of the attendees of Dr. Pons seminar at the University of Utah on 3/31/89. (reprinted w/o permission, but it was freely distributed)

¬¬¬¬¬¬¬¬¬¬¬¬begin text¬¬¬¬¬¬¬¬¬¬¬¬¬¬¬¬¬

BACKGROUND FOR NUCLEAR FUSION SEMINAR
FRIDAY, MARCH 31, 1989
2008 HENRY EYRING CHEMISTRY BUILDING

An article written by Drs. B. Stanley Pons and Martin Fleischman describing their nuclear fusion research at the U of U has been accepted for publication by the "Journal of Electroanalytical Chemistry." The article is expected to appear in the publication in late April or early May.

In the article the researchers state: "We conclude that the conventional deuterium fusion reactions are only a small part of the overall reaction scheme and that other nuclear processes must be involved."

There is not yet a complete understanding of where the heat is coming from. Fusion occurs in the cells but fusion reactions do not account for all the heat that is observed. As we stated at the press conference last week and on several occasions since then, the investigators believe that no chemical reaction can account for the heat output so they attribute it to nuclear processes.

Evidence for nuclear fusion includes; generation of heat over long periods that is proportional to the volume of the electrode and reactions that lead to the generation of neutrons and tritium which are expected by-products of nuclear fusion.

The researchers have also co-authored and submitted a second article to "Nature" for consideration for publication

Dr. James J. Brophy
Vice President for Research
University of Utah

¬¬¬¬¬¬¬¬¬¬¬¬¬end text¬¬¬¬¬¬¬¬¬¬¬¬¬¬¬¬¬¬

What follows is a summary of my notes from the lecture by Dr. Pons. Due to limited seating, I watched the lecture on a projection TV. Not very good resolution, so I missed some of the equations, but I think I got most of it. Also the physicist in our group didn't get a seat in either lecture h all and was not able to verify my notes/impressions. He did look at my notes with me though and helped clear some things up.

Electrochemically Induced Fusion

By Dr. B. Stanley Pons

Dr. Pons began with a brief history of the work began by he and

Fleischman. Initially, their interests were in the development of a metallic hydrogen material for use as a semiconductor. They realized that immense pressures were required in a lattice for this to occur. However, they theorized that it would be possible to bring about the equivalent of this immense pressure by electrochemical methods. From these initial musings, they also considered whether this "electrochemical pressure" could be used to fuse like nuclei (deuterium).

The initial experiment used a cube of Pd (size not stated) in D2O at high current density (again not stated). A Geiger counter was used to detect any radiation from the fusion reaction of D. However no radiation was detected. The experiment was discontinued by reducing the current density, and shortly thereafter (overnight I think is what he said) the experimental apparatus was vaporized. Left approximately 1/10 of the initial Pd.

Current apparatus uses a Pd rod in 0.1M D2O in a cell which has been widely seen in the media. It consists of a Pd rod surrounded by a Pt coil in a special made glass container. There are openings for charging and adding D2O, measuring temperature, and heaters. The use of rod gives better control of the surface to volume ratio. During electrolysis of the D2O the following reactions take place:

```
                 D2O + e-   <--->   Da + OD-
                        Da  <--->  Dlat
             Da + D2O + e-  <--->  D2 + OD-
```

where Da is deuterium adsorbed on the surface of the Pd rod, and Dlat is deuterium diffused into the lattice of the Pd.

Before the surface of the electrode is saturated with Da, the D diffuses into the lattice of the Pd. The evidence suggests that the deuterium diffuses into the lattice as deuterons and electrons. The electrons go to the k band of the lattice.

Dr. Pons stated that the potential of this electrochemical couple is 0.8V. In terms of pressure to get the same degree of difference in chemical potential = 10**27 atmospheres.

Dr. Pons explained a control experiment where they used a closed cell to detect tritium (else some tritium would be lost as by exchange with D2O). Tritium was detected, and its concentration increased over time. Also the neutron flux was measured as 10**4 n/s. This is 3X higher than background and was considered statistically significant. However, the reactions to produce tritium and 3He do not explain the amount of heat produced.

In this same vein, he pointed out that their experiments indicated that the heat produced was proportional to the volume of the electrode used, not the surface area of the electrode. This indicates that the process is not electrochemical in nature. An energy density of 26W/cc of electrode was calculated. One experiment produced 4MJ of heat in 120 hours. He reiterated that this could not be due to any known physical or chemical process. Since the fusing of deuterium is only part of the overall reaction scheme, other as yet unknown processes produce the rest of the heat which is detected. Dr. Pons believes these unknown processes must be nuclear processes.

He also surmised that the deuterons existed in the Pd lattice as a low temperature plasma which is shielded by electrons.

Dr. Pons then answered several questions from Faculty members (there were no microphones in the room with the graduate students where I was). The content of his responses are summarized below.

This reaction is diffusion controlled, with the diffusion coefficient for deuterons in Pd given as 10¬-7 cm¬2/s.

The production rate of tritium was found to match that of the neutrons.

Although the cross-section of Pd is too small to allow for significant reaction with energetic neutrons, it may react with neutrons back-scattered from the heavy water. No assay of the Pd electrodes has been undertaken to check for activation by-products of Pd.

The ignition/vaporization of the initial experiment was caused by a steep concentration gradient of D+ as the current density was decreased. This gave rise to compression (even greater than *normal*) as the D+ species moved out from the lattice in a radial direction. This "shock" resulted in the vaporization.

No 2.45Mev neutrons were detected. He speculated that these neutrons may be consumed by reaction with Li:

7Li + n + 2.45MeV ---> 3T + 3He + n
6Li + n ---> 3T +3He + 4.5MeV

The concentration of the deuterons in the Pd lattice is greater than 0.67 (deuterons/Pd atoms) and is estimated to be 1.0 - 1.2. They are believed to cluster at the octahedral sites in the Pd (Pd has a face centered cubic crystal structure).

In looking for products of fusion, 3He was not seen but 4He was. Part of the reason for not seeing 3He is due to the apparatus used (apparently not very airtight) and instruments used.

Other metals (which were not specified) were tried as electrodes but no heat was detected. Radiation was not monitored.

No experiments have been carried out in magnetic fields to determine quadrupole effects. He admitted that spin-spin interactions could have an effect.

The reaction is diffusion controlled. In a 0.4 - 0.5mm rod with X=10¬-7 cm¬2/s, the time required to start the reaction is (0.2)¬2 / X .

He did not know the effective mass of the electron carriers in the Pd matrix.

He felt that the addition of hydrostatic pressure to the cell would have a negligible affect on the rate of the reaction. The potential gradient at the D2O Pd interface is on the order of 10¬12 V/m. This gradient can not be achieved in gas or vacuum phase conditions.

They have recently achieved a 1W in 10W out energy ratio.

Essentially no neutrons or tritium are detected until the fusion process begins.

He jokingly predicted that 100 years would be needed to bring this technology to commercial use.

He admitted that the results were just as puzzling to him as they are to many others. He openly admits that much more work is needed to understand this phenomenon. (He did not seem to resent any questions, and was honest in his responses.)

He ended his talk with a WARNING. Please do not DO NOT attempt to repeat this experiments until you have read the journal articles or have consulted with Drs. Pons or Fleischman directly. The initial experiment which vaporized is no joke. Please consult with them or wait for the articles to appear before you begin a possibly dangerous experiment. Please act responsibly in this regard.

Please remember, these are my personal notes taken during a lecture presented in less than optimum conditions. If there are any gross errors, they are probably my fault. As I said, I briefly went over these notes with a physicist from or lab, and he did not point out any glaring errors. Nonetheless, the information presented is essentially that presented by Dr. Pons. No sound or video recordings were allowed, so the opportunity to check my notes was limited. In other words please don't blame me.

ch-tkr@wasatch.utah.edu

End of append -- YKTVMX(GILBT)

----- FUSION FORUM appended at 15:58:46 on 89/04/03 GMT (by SOREFF at FSHVMFK1)
subject: electrochemical safety question
Even if the Utah fusion reactions are quite real, I wouldn't see cause for concern in most large scale electrochemical processes. Almost any large scale process will use ordinary (protonated, not deuterated) solvents and other chemicals, and shouldn't cause D-D fusion under almost any circumstances. The fusion of ordinary hydrogen that takes place in stars is intrinsically a much slower process (I haven't ever heard anyone propose trying to run it in a fusion reactor) basically because you must turn protons into neutrons at some point, which requires an inverse beta decay, which requires the weak interaction, which gives you low rates/small cross-sections. One exception would be any process that tends to concentrate deuterium (but it has to be pretty extreme since the initial concentration is 1/6000) or that starts with a high concentration of deuterium for some reason (welding pipes in a CANDU reactor?). Lots of research uses involve some sort of high concentration of deuterium, however: NMR, IR, neutron scattering crystallography (but there you know you have to be careful anyway), stable isotope labelling techniques...

-Jeffrey Soreff

----- FUSION FORUM appended at 16:10:37 on 89/04/03 GMT (by GENERAL at BCRVM2)
Subject: Platinum's Role

Everyone keeps focusing on the role of palladium to make the fusion work. But the designers of the system seem to indicate it is just as important to have a anode of platinum as well as a cathode of palladium. Does anyone have any idea why that may be important? Seems to me that the cathode material is important, but the anode could be anything. Couldn't we use an anode of gold, or copper, or &cheap_metal, just as easilly?

I realize we won't know for sure until we can duplicate the experiment and try other anodes. I'm looking for a theoretical answer as to why platinum might be important as an anode, assuming that it is.

Robert E. Lee Jr. (GENERAL at BCRVM2)

P.S. Does anyone know for sure the polarity of the electric current? I assumed the palladium was the cathode and the platinum was the anode. Is it the other way around? Does it make a difference?

----- FUSION FORUM appended at 16:58:05 on 89/04/03 GMT (by RDHARPER at SJEVM5)
Subject: Platinum's Role
Ref: Append at 16:10:37 on 89/04/03 GMT (by GENERAL at BCRVM2)

Platinum is the anode, paladium the cathode. No reason why given, although others, I'm sure, can answer that, as it must be basic. Also, titanium has been discussed as a cathode, which would make the whole thing a lot cheaper.

Regards,
RDH
09:51:30 PST

* * *

04/03/89

----- FUSION FORUM appended at 18:05:48 on 89/04/03 GMT (by RLG2 at YKTVMV) ---
Fleischmann-Pons preprint of 03/11,20/89 for J. Electroanalytical Chemistry states (page 3)

"Stirring in these experiments (and those listed under 1) was achieved, where necessary, by gas sparging using electrolytically generated D2."

My previous append suggests that explosive energy releases might be due to disrupture of the PdHx pressurized by electrolytic injection of D. But a continuing heat evolution might be caused by air injection with the sparging D2, catalytically recombining on the Pd surface.

Dick Garwin

----- FUSION FORUM appended at 18:55:47 on 89/04/03 GMT (by RVFIRTH at CLTVM3)
Subject: Platinum's Role
Ref: Append at 16:10:37 on 89/04/03 GMT (by GENERAL at BCRVM2)

Presumably, platinum is used to have a relatively inert anode material. An anode material that oxidizes as a result of the electrolysis would play hob with the carefully prepared solution. Gold would probably also make a suitable anode material; copper would not.

Rowland Firth

----- FUSION FORUM appended at 18:59:31 on 89/04/03 GMT (by ZIEGLER at YKTVMV)
Re: Fleischmann and Pons CNF paper.

I have a copy of the paper "Electrochemically Induced Nuclear Fusion of Deuterium", by M. Fleischmann and S. Pons, submitted to the Journal of Electroanalytical Chemistry. The paper is a rather complete exposition of their work.

It is available for copying in my office, 28-024, in Yorktown. Just take it and return it after copying.

J. F. Ziegler

NOTE : Copies of the F & P paper are available also from:

Jim White	Charles Freund	Joseph Gordon	Timothy O'Gorman
65C/202	B8A/0082/ZIP 1033		E61/966-2
IBM-Charlotte	IBM-Austin	IBM-Almaden	IBM-Burlington

----- FUSION FORUM appended at 07:48:12 on 89/04/04 GMT (by ACCART at LGEVMD) -
Subject: Scientific American Feb 89. P 62 The chemical effects of ultrasound.

This paper describes effects such as very high pressure and temperature created by ultrasound on very small areas (more than 5000 degrees Celsius etc..). The paper shows physical and chemical effects, able to modify molecular structure.

My question is: Were ultrasound waves used in fusion experiments at room temperature on palladium electrode? Can they help? Is the energy level high enough? Accart at LGEVMD

----- FUSION FORUM appended at 15:49:39 on 89/04/04 GMT (by HORKANS at YKTVMV)
..... FUSION FORUM modified at 19:25:54 on 89/04/04 GMT (by HORKANS at YKTVMV)
Re: Platinum's role

In the original submission of this item, I confusing left out the word NOT in defining an inert anode. Here is another try.

The question of why Pt is used as the anode has recently come up, and indeed was adequately answered -- it provides a sufficiently inert material. I want to expand on this slightly.

Remember that we are dealing with a circuit, in which electrons are provided at the cathode and consumed at the anode. The cathode reactions are adsorption and absorption of deuterium in Pd and generation of D_2 gas. If you have an inert anode (by which I mean that it does not dissolve) then the anode reaction is generation of O_2. In other words, you are generating an explosive mixture of gases, and there is a simple chemical safety problem whether or not you achieve a fusion reaction.

Jean Horkans

----- FUSION FORUM appended at 16:27:59 on 89/04/04 GMT (by GDPINKS at DALVM21)
Subject:RTF & Vaporization

One append referenced either the paper or a speech, where the Utah researcher reported that the increase in current (in an an overnight experimental change of conditions) resulted in the 'meltdown', but what interested me was the statement that in the morning "background radiation was 3 times normal".

What does the release of the radiation prove, in terms of achieving some fusion result ? Or a fission result ?
Thanks
Dumb, but curious Glenn

----- FUSION FORUM appended at 13:29:51 on 89/04/05 GMT (by GENERAL at BCRVM2)
Subject: Platinum's role
Ref: Append at 15:49:39 on 89/04/04 GMT (by HORKANS at YKTVMV)

So, let me see if I have this straight. Platinum is good because it is inert, and won't react chemically with the "witches brew" solution.

So, we should be able to get away with an anode of &cheap_metal that has a good platinum coating on it. We don't need a solid platinum anode in the same way we need a solid palladium cathode. Nothing of interest is happening in the center of the platinum, right?

Robert E. Lee Jr. (GENERAL at BCRVM2)

----- FUSION FORUM appended at 14:19:13 on 89/04/05 GMT (by SOREFF at FSHVMFK1)
Taken from outside nets:

Date: 31 Mar 89 21:00:59 GMT
From: vac@sam.cs.cmu.edu (Vincent Cate)
Article-I.D.: <4616@pt.cs.cmu.edu>

The following 3 papers were referenced in the BYU paper and I highly recommend them:

1) "Piezonuclear fusion in isotopic hydrogen molecules"
   Journal of Physics G 12, 1986, p 213-221
   C DeW Van Siclen and S E Jones

2) "Muon-catalysed fusion revisited"
   Nature, vol 321, May 8 1986, p 127-133
   Steven Earl Jones

3) "Cold Nuclear Fusion"
   Scientific American, 257, July 1987, p 84-89
   Johann Rafelski and Steven Jones

The first paper derives the rate of fusion of deuterium. The second two discuss using muons to increase the fusion rate.

After reading these papers and their recent paper, I think there is no question that Rafelski and Jones are legitimate. It seems that cold fusion is not at all new. It was "first suggested on theoretical grounds by F. C. Frank and Andrei D. Sakharov in the late 1940's. The first experimental observation of muon-catalyzed fusion came by chance a decade later. ..."

The obvious question is, "Why is this only getting news coverage now if its been around for so long?" It seems that until the Pons nobody claimed to be able to get more energy out of cold fusion than they put into it. What's new is not cold fusion, but cold fusion that has a net energy production. Over the last 40 years people have gotten closer to breakeven cold fusion and now it seems Pons may have crossed this point.

Can people recommend any other references (from the BYU paper or otherwise) on cold fusion?

-- Vince

Date: 3 Apr 89 21:37:30 GMT
From: smb@ulysses.homer.nj.att.com (Steven M. Bellovin)
Article-I.D.: <11402@ulysses.homer.nj.att.com>

According to the Associated Press and the New York Times, the Hungarian news agency MTI has reported that two physicists at Lajos Kossuth University have succeeded in reproducing the Pons/Fleischmann results. The rector of the university has confirmed the news agency report.

They claim to have observed neutron emission, and to have confirmed this by subsequent control tests''.

----- FUSION FORUM appended at 14:57:22 on 89/04/05 GMT (by ZIEGLER at YKTVMV)
Re: Brookhaven Nat'l Lab. Confirms CNF Experiment

Wall Street Journal, April 5, 1989

"Scientists at Brookhaven National Laboratory said they have tentatively confirmed the discovery of room-temperature fusion by Bringham Young University scientists.

"We're not absolutely certain, but we have detected neutrons produced by fusion reactions that are consistent with the Brigham Young result," said Kelvin Lynn, a researcher at Brookhaven, a federal government-supported lab in Upton, NY. The neutrons were produced in a battery-like device similar to ones employed in so-called cold fusion experiments by two Utah teams, whose reports have sparked world-wide fusion fervor ......"

COMMENT:

No results reported for a Flieschmann-Pons type of experiment. Hence, what they see is the very low level neutron radiation without any heat. No experimental details given. But since such public announcements are given through official channels, we must assume the full scientific credibility of Brookhaven stands behind the announcement.

J. F. Ziegler, IBM-Research, Yorktown

----- FUSION FORUM appended at 16:36:53 on 89/04/05 GMT (by ZIEGLER at YKTVMV)
Re: Brookhaven Confirms RTF

An append above reports the Wall Street Journal saying that Brookhaven scientists have successfully confirmed the RTF of Brigham Young. The scientist quoted in the article, Kelvin Lynn, is (conveniently) away on travel today, but his neighboring physicist, Peter Bond, said that Brookhaven made NO such official announcement, and the article was without official sanction. Further, he said he had been involved in the fusion experiments, and it had a null result so far.

J.F.Ziegler, IBM-Research, Yorktown

----- FUSION FORUM appended at 16:51:58 on 89/04/05 GMT (by HORKANS at YKTVMV)
Re: Platinum's role

I have been reading the Fleischmann and Pons reprint, and I want to amend my previous comments about inert anodes. The authors speak of sparging with D2, and it seems from their comments that this is in order to stir the solution. But sparging with D2 problably plays a more important role in changing the anode reaction. In a (stirred) D2-saturated solution, the anode reaction is no longer O2 evolution but rather becomes D2 oxidation (D2 + 2OD- = 2D2O + 2e). This is the reverse of one of the main cathode reactions. Having this reaction rather than O2 evolution avoids an O2/D2 mixture in the evolved gas, minimizes D2O consumption... But the anode has to be a good catalyst for the reaction, and Pt certainly meets this criterion.

To answer a recent question, yes, only the surface of the Pt is thus important. But at this stage, before the fusion reaction is confirmed, I can't see any point in worrying about the economics of the anode. A Pt foil anode is only a tiny portion of the expense of the experiment.

As far as making LiOD from Li and D2O, PLEASE DON'T. I have a friend who lost a hand in an explosion resulting from a considerably less reactive combination of alkali metal and proton donor.

Jean Horkans

----- FUSION FORUM appended at 17:57:01 on 89/04/05 GMT (by SCHORMAN at TOROLAB2)
..... FUSION FORUM modified at 13:34:35 on 89/04/06 GMT (by SCHORMAN at TOROLAB2)
Sorry didn't read the second paragraph of the first append.

Steve Schormann

----- FUSION FORUM appended at 18:08:33 on 89/04/05 GMT (by MMFARROW at ALMVMC)
Subject: Lithium plus D2O
Ref: Append at 16:51:58 on 89/04/05 GMT (by HORKANS at YKTVMV)

I disagree. The reactivity of the alkali metals increases with atomic number, and lithium is RELATIVELY less reactive. Same for alkali earths- Be, Mg, Ca, Sr: Be and Mg are ok (probably solubility of the oxide helps to protect them) but Ca and Sr become increasingly violent.

This doesn't mean one should be casual about forming LiOD, but the reaction is slow and with sufficient air flow to eliminate any D2 gas, it doesn't have to be dangerous.

Mike Farrow

----- FUSION FORUM appended at 03:48:22 on 89/04/06 GMT (by HICKS at RCHVMV) --
Subject: Lithium plus D2O
Ref: Append at 18:08:33 on 89/04/05 GMT (by MMFARROW at ALMVMC)

Re the lithium, and the fact that the power output seems too large relative to the neutron output, is it possible that there are other fusion reactions going on other than the two D+D ones -- eg, a D+Li or He+Li? I can't find a table of isotopes, but judging from a table of atomic weights, there are several possible candidate reactions. And, if I recall correctly, fusion is theoretically profitable until some point near the middle of the periodic table (although less profitable as weights go up). Hopefully, someone will analyze the reaction products soon and let us know what is being produced.

Also, I was wondering if there might be some "chain reaction" effect: A lot of hydrogen (both H and D) is confined in the Paladium matrix, and either the heat or neutrons from one reaction could create another.

Dan Hicks

----- FUSION FORUM appended at 13:21:48 on 89/04/06 GMT (by HUYCK at RALVMG) --
Subject: Heisenberg affect on Deuterium energy

When the deuterium is occupying the interstitial sites in the palladium, each nucleus is highly confined. Question: is the confinement so small that the energy of the nuclei is spread out over a wider range of values (in compliance with Heisenberg's uncertainty principal)? Perhaps this could increase the probability that two nuclei can approach close enough to tunnel through the Coulombic barrier?

Warren A. Huyck

----- FUSION FORUM appended at 16:26:11 on 89/04/06 GMT (by ZIEGLER at YKTVMV)

Re: IBM efforts in Cold Nuclear Fusion

There are many groups within IBM trying the CNF experiments of Jones or Fleischmann and Pons. To date no one has seen any radioactive particles. We have been in contact with the groups at Bell Labs, Brookhaven, Sandia, Los Alamos, etc. all of which have several groups working on reproducing the results and all have seen ZERO.   No heat.   No particles.

The IBM - Yorktown effort has been centered on detecting either the particles or the heat. I will describe our particle experiment below. This work is the collaboration of a large group of people and not that of any single person.

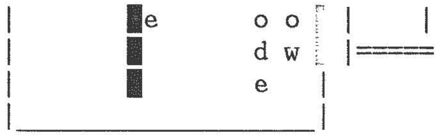
The main problem we saw with both the Utah efforts was that they were not looking at the right nuclear events to give them optimum signal to noise. The d-d fusion gives four particles : n, p, t and He as described above in Historical Perspective. All these particles have high energies, about 1 to 3 MeV. The neutron can also react with other atoms such as protons in the water bath surrounding the F-P experiment, and this will give a gamma ray (at 2.2 MeV).

The Utah people looked at the neutrons and the gamma ray, and somewhat at the tritium. When you look for neutrons and gamma-rays you always have a large background from cosmic rays and from radioactive materials such as potassium in concrete. The advantage of looking at these radiations is that there are simple commercial detectors available for monitoring this radiation. But when you are only getting a few counts an hour, such as in the Utah experiments, you are susceptible to signals from many other sources of radiation.

Our approach has been to look at the other particles, the p, t, and He ions, with energies from 1 - 3 MeV. There are NO background environmental radiations which give a peak in this energy range (alpha particles always have initial energies above 4 MeV). So we have set up an electrolytic cell with the following geometry:

```
              Electrolytic Cell

|       █           |
|       █           |
|       █A      C W ░|=====
|       █n      a i ░|    |
|       █o      t n ░|Si  |______>>> Signal >>>>
|       █d      h d ░|Det.|
```



The anode is a standard planar sheet of metal, and the cathode is a thin metal film which covers a hole in the wall of the electrolytic cell (the cell is made of teflon). The anode is either Pt or Au, about 1 x 4 cm foil. The cathode is a Pd foil, about .5 to 2 mils thick, 1.6 cm2. The cathode foil covers a hole in the wall of the cell. Outside this window is a silicon particle detector. In 24 hours, the background in this detector is about one count for particles between 1 - 3 MeV. In the d-d fusion reaction, the protons (3.02 MeV) will reach the detector from anywhere in the Pd cathode. The t and He particles will reach the detector if the fusion occurs in the bulk of the Pd material (the F-P paper concludes that he fusions occur throughout the bulk of the Pd).

The solid angle and efficiency of the detector are such that about 20% of all d-d fusions within the Pd will be detected. That is, if there are only 24 fusions in 24 hours, we will see 4 with a background of 1 particle. From both the Utah papers, we should more than 10,000 fusions/day with our cell and see 2000 particles.

We see ZERO particles with experiments which exactly duplicates each of the Utah cells. The only missing item is the "weeks" of fermentation which Pons says may be necessary in preparing the Pd samples (he keeps a constant batch of Pd samples at low voltage in D2O for months).

There are a few items I have omitted from the above description which we don't want to reveal here. This work is the result a wide collaboration of people at Yorktown.

J. F. Ziegler, reporting for the Yorktown CNF group.

----- FUSION FORUM appended at 16:31:23 on 89/04/06 GMT (by A17GSK at FSHVM2) -
Re: profitable until some point near the middle
Ref: Append at 03:48:22 on 89/04/06 GMT (by HICKS at RCHVMV)

That point is iron, atomic number 26--I forget which isotope, but probably 56. (Aside: That's why iron is so plentiful.)

--Glenn

----- FUSION FORUM appended at 20:12:01 on 89/04/06 GMT (by HORKANS at YKTVMV)
Re: TV interview with Pons 4/6

According to information passed on to me through Ron Waldron at FSD Headquarters in Bethesda, there will be an interview with S. Pons tonight 4/6 at 8:00 on WETA TV in the Washington DC area.

Jean Horkans

----- FUSION FORUM appended at 22:15:27 on 89/04/06 GMT (by MMFARROW at ALMVMC)
Subject: TV interview with Pons 4/6
Ref: Append at 20:12:01 on 89/04/06 GMT (by HORKANS at YKTVMV)

Does anyone know what that means to those of us outside the "Beltway"? Is this a network feed?

Is that PBS McNeil Leher Newshour or what?

Thanks,

Mike Farrow

----- FUSION FORUM appended at 10:06:28 on 89/04/07 GMT (by TRUEMANP at WINVMA)
Subject: Making it work better

One is tempted to ask: If muons can catalyse fusion of gaseous molecular deuterium, and if absorbtion into solid palladium can bring deuterium nuclei close enough together to enable fusion through tunneling, might not firing muons at the palladium speed up the rate of fusion?

If so, then
1) I should've spoken to a Patent attorney before writing this :-)
2) Experiments in different locations might yield very different results according to how well the cell is shielded from cosmic ray muons.

Philip Trueman

----- FUSION FORUM appended at 20:03:03 on 89/04/07 GMT (by NORD at MANCSP1) --
Subject: TV interview with Pons 4/6
Ref: Append at 22:15:27 on 89/04/06 GMT (by MMFARROW at ALMVMC)

Well, Nobody in the know has appended a summary of last night's WETA interview with Pons, so I'll give it a shot.

Overall review: Disappointing. Those of you "outside the beltway" missed very little.

Maybe I knew too much before the interview, but the technical aspects of the potential discovery were not sufficiently discussed.
Only the first 3 minutes of the interview discussed the experiment.
The remainder was spent asking Mr. Pons how he could prove his findings and asking him to refute the statements of some physicists who are skeptical of his experiment.

For me, knowing that the experiment is not confirmed is OK.
Enough said, lets move onto the technical aspects.
If the experiment is not reproducable, we will know soon enough.
A technical discussion of the experiment and the potential society benefits would have been quite nice.

Oh well, hopefully other interviews will follow.

--- Joe Nord SID/FS Manassas, VA

----- FUSION FORUM appended at 21:38:22 on 89/04/07 GMT (by PDC at SJEVM5) ----
Subject: IBM efforts in Cold Nuclear Fusion
Ref: Append at 16:26:11 on 89/04/06 GMT (by ZIEGLER at YKTVMV)
You mentioned using Pd foil. Pons has stated that foil won't work, you MUST use a rod?

Paul D. Chamberlain

----- FUSION FORUM appended at 23:23:29 on 89/04/07 GMT (by VENDOR26 at PALVMPSQ)
Subject: IBM efforts in Cold Nuclear Fusion
Ref: Append at 21:38:22 on 89/04/07 GMT (by PDC at SJEVM5)

You also mention a window in the cell. As Bob Miller mentioned earlier, we are dealing with very high D/Pd ratios. Couldn't the exposed side of the cathode allow deuterium to diffuse out into the air, dropping the D/Pd ration to a point where fusion won't occur ?

Dick Murtagh

----- FUSION FORUM appended at 12:20:41 on 89/04/10 GMT (by MITCH at LEXCJN1) -
Subject: NPR Report

National Public Radio reported this morning that Texas A & M has "duplicated" the University of Utah experiment and would hold a news conference later this morning. NPR did not say they would cover it or not.

Gary A. Mitchell

----- FUSION FORUM appended at 12:39:13 on 89/04/10 GMT (by RVFIRTH at CLTVM3)
Subject: IBM efforts in Cold Nuclear Fusion
Ref: Append at 16:26:11 on 89/04/06 GMT (by ZIEGLER at YKTVMV)

This thought has no doubt already occurred to you folks in Yorktown, but on the off chance that it hasn't: Suppose there is something to the theory that fusion occurs by tunneling only after all of the interstitial sites are occupied. According to this theory, the incoming deuterons are briefly occupying the same energy states as those already in the lattice since there are no other available locations. If this is true, the form factor of the palladium cathode, especially the surface area/volume ratio and absolute thickness is likely to be critical. If the deuterons are easily able to tunnel out of the lattice, into the solution or into your window, then fusion may be greatly reduced. If the above is occurring, a large "if" admittedly, then thin foil would be the worst possible choice for a cathode. A large sphere would be more fusion efficient, perhaps too efficient judging by the results of the cube experiment (although the "meltdown" was probably a conventional chemical reaction, not excessive fusion).

Rowland Firth

----- FUSION FORUM appended at 13:52:01 on 89/04/10 GMT (by RMILLER at YKTVMV)
Re: Does Li react violently with Pd?

This note contains speculation relating to the role of the lithium (from the LiOD electrolyte) in the Pons and Fleischmann observation that "a portion of the cathode fused (melting point 1554 C), part of it vaporized and the cell and contents...were destroyed" as a result of the heat of the reaction in one instance.

According to Moffet's handbook on binary alloys, the phase diagram of Pd-Li includes Pd-rich phases at Pd(7)-Li, Pd(2)-Li and Pd-Li, as well as several Li-rich phases. The melting points of these phases are ca. 1500 C, ca.950 C, and ca. 650 C, respectively. So melting at substantially lower, but nonetheless impressive temps could have occurred if Pd-Li alloys had formed during the electrolytic charging.

Elliott's supplement to Hansen's Constitution of Binary Alloys does not include Pd-Li, but does contain the following note about Pt-Li: "contamination-free Li and Pt react in a violent exothermic reaction at 540 C in vacuum or an inert atmosphere to form the compound LiPt2, which is FCC ...." The phase diagram in Moffet for Pt-Li has the same M-rich phases as the Pd-Li phase diagram, where M is Pt or Pd (melting points not given for the Pt-Li phase diagram).

Would it be conceivable that a Li-Pd "violently exothermic" reaction occurred in the Pons and Fleishmann experiment, as occurs at higher temp in the Li-Pt system? The electrolysis occurring at the cathode would be a reducing environment, promoting contamination-free surfaces in the sense of being free of oxide surface layers, perhaps thereby promoting the reaction.

SAFETY NOTE: If a strong exothermic reaction is occurring between Li and Pd during electrolytic charging, then the reaction could occur even using LiOH instead of LiOD. If fusion is occurring with the LiOD, we would not expect it with the LiOH, so one would expect to be safer from the perspective of nuclear reaction products using the LiOH, but the likelihood of the Li-Pd reaction is unchanged.

Bob Miller, Yorktown

----- FUSION FORUM appended at 17:00:23 on 89/04/10 GMT (by ROBERT at LOSANGEL)
Subject: Heavy Water

I acknowledge I have no right to to append to this FORUM since I am not an expert. I thought, however, that something I heard over lunch over the weekend might be of interest to the experts.

My wife and I had lunch with a chemist/physicist and his wife. As we we were talking about what 'we do', I learned that my guest, Mr. Robert Cohen, is President of Enterprise Chemical Corporation and his group has developed a catalyst to be used in the cheap/efficient manufacture of heavy water. They have acquired American patents and are currently trying to get international patents. An American company - Air Products, and Canadian - Ontario Power, have expressed interest in their catalyst because of the use of heavy water as a moderator in nuclear fission reactors.

I'm sure IBM has no interest in going into the business of manufacturing heavy water, but if any of you guys has an interest in this catalyst, Rob promised to send me a copy of his paper. Anyone?

Regards,

R.S. (Bob) Prentice - LOSANGEL/ROBERT

----- FUSION FORUM appended at 17:20:56 on 89/04/10 GMT (by NAMEROW at POKVMCR3)
Subject: NPR Report
Ref: Append at 12:20:41 on 89/04/10 GMT (by MITCH at LEXCJN1)

Did the news conference take place yet ??? What did they say ??? Their ability to duplicate the experiment is VERY exciting ! I am growing more and more (cautiously) optimistic...

-Wayne

----- FUSION FORUM appended at 19:37:47 on 89/04/10 GMT (by MFC at VENTA) -----
ITN 7pm news tonight reported that the Texas group reproduced the Fleischman/Pons experiment at 180% return. (That is, 80% more energy out that in), but still have no statement as to whether fusion or chemical reaction. Any more details from USA?

Mike Cowlishaw - IBM UK

----- FUSION FORUM appended at 20:28:44 on 89/04/10 GMT (by GORDON at ALMVMC) -
Subject: Heavy Water
Ref: Append at 17:00:23 on 89/04/10 GMT (by ROBERT at LOSANGEL)

Cold Fusion verification at Texas A&M

I just talked with a chemistry faculty member at Texas A&M, not the one who reported verification of the F&P result. What the A&M team reported was that they had observed excess heat from the electrolysis of D2O on Pd. My second hand contact said "up to 90%". That is 1.9 times out what went in. They have not observed a corresponding flux of neutrons. Chuck Martin is the electrochemist in the group and he is working with two people from the Center for Thermodynamics, so the calorimetry is likely to have been done correctly.

Note, they simply verify excess heat and make no claims as to the source.

Joseph G. Gordon II

----- FUSION FORUM appended at 20:52:12 on 89/04/10 GMT (by GORDON at ALMVMC) -
Subject: Heavy Water
Ref: Append at 17:00:23 on 89/04/10 GMT (by ROBERT at LOSANGEL)

I just talked to a post doc, Frank (forgot last name), in Chuck Martin's group at Texas A&M. They see excess heat from the electrolysis of D2O at a Pd wire. Out = 1.67 x In.
Assuming no recombination of O2 and D2, Out = 1.8 x In.
(I.e. subtract 1.5V from the total cell voltage to correct for the enthalpy stored in O2 + D2 gas which is not measured by the calorimeter.)
They have ascertained, by measuring the volume of the gases given off that there is little recombination.

They used a 1mm by 5cm wire and see results within one half hour after turning on the current. Output has been sustained for 48 hours.
Electrolyte composition is .1 M LiOD in D2O.
Joseph G. Gordon II

----- FUSION FORUM appended at 21:31:07 on 89/04/10 GMT (by MACNAIR at RHQVM08)
Re: Wall Street Journal Article Today (4/10)

There's a brief article in today's Wall Street Journal (page B2) which confirms that Texas A & M University Researchers have duplicated a University of Utah experiment. The article says that "School officials said that they would hold a news conference today to announce that their researchers had achieved the same kind of cold fusion".

I suspect that there's still going to be considerable doubt about what is really going on, since the spokesman went on to add "Our people are convinced that they've got it, so as a gesture to the researchers in Utah we wanted to announce it as soon as possible." Obviously, this confirmation is being rushed out. I suspect that all they will confirm is that they are seeing some abnormal heating, and that they really haven't done much in the way of measurements to confirm any fusion reactions (eg gamma rays or neutrons or fusion byproducts such as tritium).

Jim Mac Nair

----- FUSION FORUM appended at 09:39:44 on 89/04/11 GMT (by MFC at VENTA) -----

Re: A report from CERN

Today received the following report of a seminar at CERN.

Mike Cowlishaw - IBM UK

....

From: MORRISON%VXPRIX.decnet.cern@cernvax

31 March 1989.

PHYSICS NEWS - COLD FUSION?

There have been many reports in the newspapers that Prof. Fleischmann of Southampton and Dr. Pons of Utah have evidence for cold fusion of deuterium by electrochemistry. This afternoon Prof. Fleischmann gave a seminar in CERN. Because of the many media reports, the auditorium was crowded and although I arrived 20 minutes early, I had to sit on the steps. As I have given several lectures on Wrong Results in Physics, I went to this and also to the press conference afterwards - especially as the news reports had been very hard to understand scientifically, but if true, this could have a major impact on the world economy.

Martin Fleischmann had a reputation as a major expert in his subject. As his talk developed, it became clear that he was a first class scientist and it seems to me that he has made a major breakthrough, though what the fundamentals processes are is not yet fully understood.

Let me try and explain what I think I learnt (I talked to him for a while afterwards, so it may not be too bad).

Basically the catalyst used, palladium Pd, is a face-centred crystal. It can absorb a certain amount of hydrogen. If an electrical potential is applied, then over a period of time it can absorb a great deal. For F & P, they reached 0.6 atoms of deuterium per atom of Palladium after three months.

They made tests with four rods each of 10 cm length and of diameters 0.1, 0.2, 0.4 and 0.8 cm. They only have good measurements for the first three as one morning when they came in they found that the fourth and largest rod had melted and the fume cupboard was starting to smoulder! They made calorimetric measurements and found that they were getting more heat out than they had put in and this effect increased with the diameter of the rod. It seems to be a volume effect and not a surface effect. The excess heat is about 5 megajoules per cm3 which is about 100 times greater than any known chemical process.

A second measurement was by putting a NaI crystal close when they recorded gammas. The energy spectrum of the gammas was sharply peaked between 2000 and 2400 which is characteristic of the (n,gamma) reaction on hydrogen. This could be explained as the neutrons interacting in the water bath round the experiment.

Thirdly they observed tritium production and measured and found a "characteristic" spectrum (I did not understand this fully, partly as he had an incomplete scale on the graph, but see later).

Fourthly they looked for neutrons using a polythene sphere filled with BF3. The count was three times background. In 50 hours they counted 40 000 neutrons. However there is a point that is a stumbling block for particle physicists - if you take the rate of release of heat, then there should be 10 E 13 or 14 neutrons - a huge discrepancy. He does not have the equipment to measure the neutron spectrum - the neutrons have to pass through the surrounding water bath which tends to thermalise them.

A conclusion that can be drawn from Fleischmann's talk is that the heating is not due to the reactions

$$2D + 2D \longrightarrow 3He + n \qquad (1)$$

or

$$2D + 3T \longrightarrow 4He + n \qquad (2)$$

which are the ones that spring to mind.

He gave a table of the excess enthalpy in the Pd rod cathodes expressed as a percentage of breakeven values;

0.1 mm	81%
0.2	189%
0.4	839%

From this it can be judged that it was not too surprising that the 0.8 cm rod melted!

He opened his talk with a basic discussion of electrochemistry.

$$D_2O + e^- \longleftrightarrow D(absorber) + OD^-$$

$$D(absorber) \longleftrightarrow D(lattice)$$

$$D(absorber) + D_2O + e^- \longleftrightarrow D_2 + OD^-$$

With the applied field the D can go over the potential barrier by applying a Potl. Difference at the interface. The result is that inside the Pd there can be many collisions without repulsion. Effectively there is a PD of 0.8 eV which can translate into a compression of 10 E 27 atmos. i.e. it would require this enormous pressure to achieve the same PD. Thus electrochemistry is high energy chemistry! The D is in a sea of high electron density. The structural or coherent strength of the Pd is 4000 atm. Thus it is a very strange kind of Quantum Mechanics (his phrase).

(continued) 1 April 1989.
(despite the date, it is serious!)

Re-reading what I wrote yesterday. I realise that I have been trying to explain simply. The actual talk contained some more details and two tables of results that I had only time to copy down partially. There was a fuller discussion of electrochemistry.

The question now is what is happening. The observations are of a source of heat, of emision of tritium, gammas and of neutrons, but the number of neutrons are many orders of magnitude less than would be expected if the heat produced came from reactions producing neutrons. Fleischmann talks as if you have to modify quantum mechanics - this I do not believe - we have to apply it differently.

An additional piece of information that he gave at his press conference but not at his seminar, was that the particle emission was not uniform but had fluctuations which were much larger than statistical - this I think is a very important piece of information.

There are a lot of different theories being discussed. The following comments should be considered personal, qualitative and not necessarily correct.

The catalyst, palladium works by accepting an incredible number of deuterium nuclei in the spaces of its face-centred cubic lattice. The distance between each deuterium nucleus is therefore reduced. This was first demonstrated by the observation of muon-induced catalysis where, in deuterium, the electron is replaced by a muon. As the muon is some 200 times heavier, the proton and neutron are pulled closer together so that the probability of fusion is greatly increased - by many orders of magnitude. Now there are two suggestions;

1. Since the deuterium nuclei are in a very dense electron field, it may be that the electrons have an effective mass much greater than normal and this increases the probabilty of the nuclei tunnelling through the barrier.

2. the applied potential difference drives more and more deuterium nuclei into the spaces between the palladium atoms so that the separation of the nuclei decreases so that the probability of fusion increases dramatically.

Personally I have a preference for the second approach, but it is always possible that both are applicable.

Instead of saying that there is a discrepancy between the number of neutrons produced and the heat produced, perhaps we should assume that all the results are correct and that the reactions ocurring are different. Maybe the dominant reaction is fusion, D + D ---> 4He, but we need something else to share the energy and momentum produced - this could be the close neighbouring structure of the lattice. Thus the dominant reaction is to produce heat! Of course other reactions will also occur which is why there is an observation of tritium and one would expect some production of 3He and 4He and neutrons and gammas. If this were true, and again this is mainly a suggestion which needs experimental confirmation, then this would have tremendous social effects as we would have a simple source of energy without the particulate matter, sulphur and other gasses from coal and oil fired power stations that are killing so many today. Also the radiation danger would be very much less than with nuclear reactors (sell your coal and oil shares if you have any!)

In answer to a question, Fleischmann said that they had tried to look at 3He and 4He production and ratio, but the experiment is difficult for them and they prefer to leave that for experts who have the equipment - for they have been using their own money for 5 years.

Looking again at my notes, I discover that John Ellis had said in the discussion that there could be little Coulomb repulsion as there could be a classical oscillation of the lattice.

Before the Seminar, things were rather disturbed with the media - lots of TV crews and flashes popping off. The Chairman, Carlo, asked them all to leave explaining this was a scientific meeting and he did not want questions on any other subject, but afterwards there would be a press conference. After some time the media left. At the end of Fleischmann's talk, the TV crews re-entered and had to be requested to leave again before the question period.

On the way to the press conference, Fleischmann was told that there had been a report on the radio that a group (at Columbia?) had confirmed his result. He said he had not heard this and during the Press Conference he continued to emphasise, in a very proper manner, that before leaping to conclusions, there should be further confirming evidence.

Fleischmann had described his other press conference in Utah as awful, but this one went well with Carlo a good Chairman - who was also asked questions. Fleischmann explained that the work was intentional and not an accident. He said that after verification, it might take 10 to 20 years to develop an economically viable system. Carlo was asked his opinion and said that "Dr. Fleischmann has planted a seed - will the seed grow up? I think yes" Fleischmann said that he believed in Karl Popper's philosophy - you cannot prove something right, you can only prove it wrong. "We have spent 5 years trying to prove ourselves wrong, now other people should try".

In explaining why they did it, "it was not to do an ego trip (though all scientists are on an ego trip to some extent), but to try and find a plentiful source of energy. We have a social conscience"

Question - "There was a sceptical atmosphere in the room, did you feel like a chemistry bull in an arena of physics toreadors?"

Answer - "Are people correct to be sceptical?, yes, it is correct to be sceptical. But it was not a bad atmosphere. Our experiment fits partly into accepted ideas but not entirely, therefore either experiment is wrong or we have extended the conceptions of possible fusion mechanisms".

Carlo was asked if he found the meeting strange - "No, I am at home in my own lab".

Question - "Do you think it is correct?". Answer(MF) - "I think it is correct, but others should show it is correct". (Note, this was typical of some of the questions where the journalist asked "for a good quote").

Carlo was asked if CERN should work on fusion. He replied " There are different science cultures. In an orchestra everyone tries to play his own instrument, and does not have other instruments. But we have quantum mechanics in common. We should do what we do best. But there is also cross-fertilisation between chemistry and nuclear physics" He also joked that this was the first time that a chemist had discovered a neutron!

Question - "Any military applications?"

Answer(MF) - "There will always be some military application of anything, but we do not know of any such thing"

Question - " You said you did not have enough money, have you been offered money since your press conference last week"?

Answer - "Up to now have used our own money as we thought it unlikely to work, so there were some restrictions. Since then we have been approached with offers but as our capacity to spend money is limited, we have to plan carefully.

Question - "If it is fusion what will its effect be on other fusion research?"

Answer - " Glad you asked that. It would be a total disaster to cut back on other fusion research. Ours is small scale, theirs is large scale generation of electricity. It would be extremely foolish to cut back".

There was more, but I hope this gives the flavour - both Fleischmann and Carlo aquitted themselves very well and responsibly.

Friedrich Dydak had told me he had two papers confirming the F & P work and I could copy them. Later when I was returning them, Fleischmann came in for another TV interview and we talked while he was waiting for the lighting to be set up. He had not seen the papers, so I gave him copies. The main author was Stephen Jones who is at the BYU in Utah beside Dr. Pons. We looked quickly at the papers - he was particularly interested in the dates on the papers. I explained I was interested particularly for two reasons. Firstly as I was possibly the first to observe fusion in Europe - in the early sixties I was scanning bubble chamber film of deuterium and normally when there is the decay chain,

pion ---> muon ---> electron

the muon always has the same short range (if the pion is at rest). But one day I observed an extra long range for the muon. I spent some time measuring the curvature and angles of the tracks, but could not explain it. However someone told me that the Berkeley bubble chamber group had found it and it had been explained as the muon replacing an electron and causing fusion. At this Luis insisted that this should be treated as a secret, but quickly it was calculated that it had no military or economical value. So I left it and went on to new things(incidently the Scientific American article of July 1987 by Rafelski and Jones on Cold Nuclear Fusion says that this muon-induced fusion was first suggested by Frank and Sakharov in the late 1940's).

Secondly I said I had given several serious lectures on Wrong Results in Physics and found that they exhibited certain characteristics so that they could be recognised before they had been proved wrong - after the press reports I wondered if this was a case in point, but after I had heard his conference, I was inclined to believe that his results were correct. He did not seem to appreciate this too much, not unnaturally, but we continued talking and he told me some remarkable things. I mentioned that after the press conference, Dr. Wind was looking for him as he used to work in Utrecht on electrochemistry and had been able to insert 1000 hydrogen ions per atom of palladium catalyst. Dr. Fleischmann (who had attained 0.6 ions after 3 months) said he did not believe this number of 1000. However talking with Per-Olaf Hulth this morning, he had checked this subject last night and read that 850 ions of hydrogen had been inserted - this could be used as hydrogen storage cells for cars driven by hydrogen - air mixtures. If I remember rightly, Fleischmann had replied that they had not prepared the surface of their palladium rod, and this could make a big difference. If it were possible to insert so many deuterium ions into palladium, then the rate of fusion would be greatly increased (or the charging time would be less than 3 months).

The two papers are;

1. "Observation of Cold Nuclear Fusion in Condensed Matter" by S.E. Jones and others of Brigham Young Univ. and J. Rafelski of Univ. of Arizona.

2. "Limits on Cold Fusion in Condensed Matter; a Parametric study" by J. Rafelski and others of Arizona and S.E. Jones of BYU.

The main point of the first paper is that they claim to have observed neutrons when there was low voltage electrolytic fusion of deuterons into metallic titanium or palladium. They believe this is from the reaction;

$$d + d \;\text{--->}\; 3He(0.82\ MeV) + n(2.45\ MeV) \qquad (1)$$

The distribution of counts in different channels give a broad enhancement which the authors say corresponds to neutrons of 2.45 MeV. This looks convincing - just; it would be good to repeat this.

They say they have not yet(?!advertising?) obtained results regarding the parallel reaction;

$$d + d \;\text{--->}\; p(3.02\ MeV) + t(1.01\ MeV) \qquad (3)$$

The electrolyte contains various minmeral salts and they say that their evidence indicates the importance of co-deposition of deuterons and metal ions at the negative electrode. "hydrogen bubbles were observed to form on the Pd foils only after several minutes of electrolysis, suggesting the rapid absorbtion of deuterons into the foil; oxygen bubbles formed at the anode immediately". The palladium pieces were 0.025cm thick and had the surfaces roughened or were mossy. They do not say that it took 3 months to get started by charging the deuterons into the palladium (private comment - this suggests to me that Fleischmann and Pons would have improved things if they had increased the surface to volume ratio of the catalyst and roughened its surface, but it is hard to be sure. However it does suggest that it is possible to charge the catalyst in much less than three months).

The experimental part of their paper gives an impression of haste, but there are a lot of other interesting things in their paper; In a deuterium molecule the separation between the deuterons is 0.74 A and the d-d fusion rate is very slow about 10 E -70 per D2 molecule per sec (calculated in an interesting paper by Van Siclen, C.D. and Jones, S.E., Journal of Physics G Nucl. Phys. 12 (1986) 213 - here they state that the fusion rates for reactions (1) and (3) are nearly equal over the range 10 to 30 KeV. They also discuss whether piezonuclear fusion - i.e. by pressure - within the liquid metallic hydrogen core of Jupiter could account for the fact that the planet radiates 1.5 times as much heat as it receives from the sun. However they concluded that this process was many orders of magnitude too small to be a significant energy source - this is where the idea of Fleischmann and Pons of using electrolytic catalysis is so important). However in muon-induced catalysis the internuclear separation is reduced by about the ratio of the muon to the electron masses (200) resulting in the fusion rate increasing by an enormous factor, 80 orders of magnitude! In the second paper this variation of fusion rate as a function of the distance is quantified. This made me think of the observation by Fleischmann that they had observed large fluctuations in the signals - for the number of deuterons in a space in the lattice of Palladium is discrete and given by Poisson statistics hence the distance between the deuterons will vary appreciably - this and other factors(roughness of surface) could cause there to be local spots hot in space and time, since the fusion rate varies so violently with distance. In addition to the reactions (1) and (3), there can occur the reaction on tritium that will exist to some varable extent,

$$2D + 3T \text{ ---> } 4He + n \qquad (2)$$

Although there is less tritium than deuterium, this reaction has a much higher cross section - so that this reaction (2) could also help fluctuations (but these comments on fluctations are my own, so treat them with appropriate caution).

Paper (1) also has an interesting chapter on Geophysical considerations (or the Hawaii effect). Sea water contains about one part in 7000 of deuterium. By subduction water is carried down to the earth's mantle where it might undergo fusion via the reaction;

$$p + d \text{ ---> } 3He + \text{gamma}(5.4\text{ MeV}) \qquad (4)$$

under the extreme pressure and temperature there. Calculations are done which indicate that a substantial contribution to the heat flux through the crust could come from cold fusion. This heat could also help to explain the localised heat of volcanism at subduction zones. They quote that the 3He to 4He ratio is high in rocks, liquids and gases from volcanoes. Further they then predict that tritium will be produced from d + d fusion and since tritium is relatively short-lived(12 years half-life), observation of tritium would suggest a geologically recent process. On the Mauna Loa mountain on Hawaii, tritium was monitored from 1971 to 1977 and a correlation is shown in the paper between the tritium level and volcanic activity. This is very striking for the 1972 Mauna Ulu eruption but later eruption signals were partly confused by atomic bomb tests. They estimate that in the Mauna Ulu eruption 100 curies of tritium was released per day for 30 days!

In paper (1), it is also reported that after diamonds are sliced with a laser, the concentration of 4He and 3He has been measured - it is reported that the 4He is distributed uniformly while the 3He is concentrated in spots suggesting cold fusion reactions. Similar anomalies have been reported in metal foils.

The authors also calculate that the excess heat from Jupiter could be accounted for from cold fusion in the core consisting of metallic hydrogen plus iron silicate.

The second paper calculates the cold fusion rate of d-d as a function of

1 - relative energy,
2 - separation of two hydrogen nuclei in a sphere,
3 - the effective electron mass,
4 - the effective electron charge.

They do not consider the effects of the lattice of a catalyst as do Fleischmann and Pons.

It is probable that some readers will be thinking that this letter has wandered off strict physics news. They are right. It is intentional as I feel this subject will become so important to society that we must consider the broader implications as well as the scientific ones. Looking into a cloudy crystal ball, it is not impossible to foresee the situation that the experiments are so easy that schools will be doing them, that many new companies will start up, most(not all) will fail and the present big power companies will be running down their oil and coal power stations while they are building deuterium separation plants and new power plants based on cold fusion. No new nuclear power stations will be built except for military needs. There will be very little if any research on high temperature(plasma) fusion. Petrol will probably still be used for cars. Overall pollution will start to be less. Ecologists will be talking about the contamination from radioactive tritium and asking about the effect of this tritium on the ozone layer.

CONCLUSIONS

It is known(from muon cataysis) that if two nuclei of deuterium or tritium are held close together, then they can fuse releasing energy.

Fleischmann and Pons thought of achieving this by using electrolysis to insert deuterium nuclei inside a palladium catalyst. They observed production of more heat than they put in. They also observed tritium production, gammas of an energy consistent with neutrons interacting with the surrounding water bath, and neutrons directly. They thus conclude they have observed fusion of heavy hydrogen producing energy, i.e. cold fusion. A paper by Jones et al. reports on the operation of similar electrolytic cells with observation of neutrons with an energy spectrum consistent with that expected from deuterium fusion. They also describe interesting though rather anecdotal evidence for fusion in volcanoes, Jupiter, diamonds and metal foils. The theory, while not fully developed, suggests that the deuterium nuclei inside the lattice of the catalyst, are held so closely together that the probability of fusion(the tunneling effect) is dramatically increased by many orders of magnitude. it may be expected that this will cause major changes in the energy industry and major social, economic and hence political changes.

Douglas R. O. Morrison.

----- FUSION FORUM appended at 12:20:43 on 89/04/11 GMT (by MPREDKO at TORVMFG1)
SUBJ: Texas A&M Experiment

Last night's news showed a clip of the Texas A&M experiment operating and (presumably) producing heat.

I was surprised to see that the experiment was emitting flashes of light.

I wasn't expecting to see any visible light emitted from the experiment, everything written here had led me to expect that the only emissions from the experiment would be neutrons and gamma rays (high energy photons).

Does anybody have any explanation for the flashes, or is it simply a warning strobe placed on the experiment warning that it is producing potentially harmfull radiation?

Myke Predko

----- FUSION FORUM appended at 13:41:17 on 89/04/11 GMT (by JQUAY at ATLVMIC1)
Subject: BYU results reproduced at Georgia Tech
Ref: Append at 19:37:47 on 89/04/10 GMT (by MFC at VENTA)

A group of scientists at Georgia Institute of Technology reportedly reproduced the results of the Brigham Young team. Source: CBS evening news 4/10/89
Jonathan Quay

----- FUSION FORUM appended at 13:43:08 on 89/04/11 GMT (by GERSON at DALNESSC)
Subject: Newspaper Reports on Texas A&M and Georgia Tech Results

This morning's Dallas Morning News had a front page report on the corroboration by groups at Georgia Tech and Texas A&M. The Georgia Tech group reported "neutron production indicating fusion"; The A&M group reported "net positive energy produced". Unfortunately, neither group did both experiments together, so there is no data verifying "neutron production" and "energy production" together.

The local Dallas press is reporting that the ACS expects a standing-room only crowd for the Wednesday noon presentation by Pons on his work. Our group plans to be there and will update the Forum accordingly.

Dennis Gerson
NIC Applications Support-Computer Aided Chemistry
E/S Nat. Support Center, Dallas

----- FUSION FORUM appended at 14:08:50 on 89/04/11 GMT (by 86604452 at WARVM2B)
Subject: Texas verification
Ref: Append at 19:37:47 on 89/04/10 GMT (by MFC at VENTA)

BBC 5 pm PM program also carried this, plus a very short interview with Fleischman, who was cautiously optimistic. He stated that the Texas experiment was important because it had been carried out under much more rigorous conditions than his. The Times (London) carried an article which stated that power out was definitely > power in, that the precise mechanism could not yet be explained, but that there was little doubt now of the fact that something was happening, and the original experiments were confirmed.
Dr. Robin Bignall

----- FUSION FORUM appended at 14:27:20 on 89/04/11 GMT (by MACNAIR at RHQVM08)
Re: Pons and Fleishman confirmed?

This is a copy of an append I sent to Newsclip forum, which I am duplicating here because Newsclip is a transient forum.
Article on front page of New York Times, April 11

Claims of Achieving Fusion in Jar Gains Support in 2 Experiments

Results from two universities (completely independent)

Texas A & M
Georgia Tech

Confirmed different aspects of experiment

Texas A & M confirmed abnormal large energy increase, but could not confirm fusion was occuring.

Georgia Tech confirmed nuclear reactions by neutron detection, but did not measure heat.

Texas A & M group led by Dr. Charles Martin

Other researchers identified as Dr. Kenneth Marsh and Dr. Bruce Gammon

Cell was enclosed in water bath to measure heat produced.

"We have been unable so far to measure any other effect of the reaction, and we're certainly not confirming at this point that we're seeing nuclear fusion. We have not ruled out the possibility that this is simply a chemical reaction of some kind. To do that wi require a great deal more experimentation than we have yet been able to perform." Quote attributed to Dr. Martin.

However, the scientists did not see any obvious explanation for the amount of heat being measured.

Dr. Martin also stated that it was far from obvious that even if a fusion reaction was taking place, it might not ever be more than a laboratory curiosity. However, it clearly warranted further exploration.

Results were partly funded by the Office of Naval Research and the Electirc Power Research Institute.

Georgia Tech group led by Dr. James Mahaffey.

"Our data convinced me that we are making neutrons in that vessel. There is no way to get neutrons unless something nuclear is going on."

Measured 13-fold increase in neutron flow when experiment was turned on. During the experiment, they measured 600 counts per hour.

Took careful steps to be sure their neutron measurements were from the reaction and not from another source. They did a control which measured flows when experiment was not running, to measure background counts.

Cathode of vessel was shielded by graphite, tap water, parafin, boron and lead.

Experiment has cost $25,000 to date.

Meeting scheduled this week in Dallas by the American Chemical Society for special discussion of the assertions.

End of summary. A few comments on the article. It certainly seems to confirm Pons and Fleishman's work, including the large excess heat production and the low neutron counts. It was unclear if Georgia Tech saw any marked heating in their experiment, and Texas A & M didn't appear to have looked for evidence of fusion (eg neutrons or gamma rays) Also unclear was if the 600 neutrons per hour was what the counter saw (eg a small fraction of those produced) or if this was their estimate of the total output. In either case, it is vastly too low to account for the kind of heat that Pons and Fleishman reported, and Texas A & M claimed to have confirmed. Also, the article contained some other discussions of fusion in general and Pons and Fleishman's work, which I have omitted.

----- FUSION FORUM appended at 15:21:26 on 89/04/11 GMT (by TEDKIRBY at BOSTON)
I haven't seen this appended yet, so....

The Boston Globe reported this morning that Georgia Tech has reproduced the Fleischman/Pons experiment, and they measured neutrons and tridium at 8 to 10 times the background level. They have verified fusion. They reproduced the experiment without having read the technical papers, based only on information from news reports. They pre-heated the anodes so that the reaction only took about 2 hours to produce fusion.

----- FUSION FORUM appended at 15:31:55 on 89/04/11 GMT (by CJKUO at LOSANGEL)
Subject: Texas A&M Experiment

Prof. John Bockris was JUST interviewed on FNN. He is a "principle investigator" of the fusion experiments at T A&M.

He says that Texas A&M has noticed heat at 60% more out than in. They have not noticed any neutrons. In addition, he mentioned that not any old Pd will work. They used Pd wire that was baked in an Argon Oven. And the wire had been "drawn and undergone a tensile stress."

(He also gave voltages and other stats which unfortunately went by before I could get the tape into the machine.)

Furthermore, he had spoken to the Hungary researchers who said that they had seen neutrons for an hour or two and then the neutrons disappeared. They then had to clean the Pd and the neutrons came back.

I suggest someone call this guy since he's speaking to the media.

Jimmy Kuo

----- FUSION FORUM appended at 15:59:42 on 89/04/11 GMT (by NAMEROW at POKVMCR3)
The associated Press released a summary of the news conference held by Texas A&M. Charles Martin, associate chemistry professor stated that they have partially duplicated the Utah experiment and were getting 'Significantly more energy in the form of heat, than they were putting into it in the form of electricity'. However they cautioned that 'Further research must be done to confirm that the excess heat generated was the result of nuclear fusion and not merely an unexplained chemical reaction.'.
There will be a meeting in Dallas later this week between the Texas researchers and their Utah counterparts.

-Wayne

----- FUSION FORUM appended at 16:08:20 on 89/04/11 GMT (by MCINNIS at AUSVM9)
Subject: Texas A&M Fusion

From the Austin American-Statesman 4/11/89

Press conference 4/10/89

Charles Martin, Ken Marsh, and Bruce Gammon report confirmation of excess heat generated in RTF type experiment. No confirmation yet of other signs of RTF. A&M researchers aren't calling it fusion yet. (They aren't denying it's fusion either.)

They haven't detected neutrons or other fusion by-products yet.

Marsh and Gammon are director and assistant director of A&M Thermodynamics research center.

The cell that worked was assembled only on Friday April 7. It started working after 40 hours.

There's a picture of the cell. It looks like a half liter beaker. It might have a 1 inch round glass pipe with a wire mesh on the outside. This pipe is oriented vertically, centered in the beaker. (I say might because of limited resolution in the picture.)

Mickey McInnis   (MCINNIS AT AUSVM9) B5 678/6779 Austin, Texas

----- FUSION FORUM appended at 16:31:51 on 89/04/11 GMT (by CJKUO at LOSANGEL)
Subject: The associated Press released a summary of the news conference held by
Ref: Append at 15:59:42 on 89/04/11 GMT (by NAMEROW at POKVMCR3)

Coincidentally, I'm going to Dallas within the hour. When and where is the hubbub going on in Dallas? -- Jimmy Kuo

----- FUSION FORUM appended at 17:09:41 on 89/04/11 GMT (by MIKCLRK at HVTVM2)
Subject: Thoughts about the reactions.

Another thought about the palladium lattice - could the reactions be centering around discontinuities in the lattice, rather that in the centre of the palladium crystals ? This would tie in with the reaction being volume dependent and, depending on how the wire was produced, might help explain the speed with which the Texas group are getting their results (and maybe the irregular rate of reaction if the fusion process is evolving enough heat to momentarily melt the lattice and it's reforming into a 'proper' lattice when it cools - this should lead to the palladium being made up of smallish, evenly distributed crystals and evolving more heat the more it's used).

Has anybody done any studies of the crystalline structure of the palladium before and after use in a 'successful' fusion experiment ?

The other reason for asking is that at the discontinuities the lattices of the two palladium crystals are off set and there will be an group of partial interstitial sites that are closer (50% ? on average) than the usual sites. They won't be very 'attractive' for duterium to diffuse into, but when there's no where else to go it'll probably end up there.

This could tie in with the 2D + 2D => 4He + heat reaction mentioned in an earlier append (how much energy would be transferred if a high energy neutron hit a palladium atom it the lattice ?).

Mik Clarke

----- FUSION FORUM appended at 17:34:53 on 89/04/11 GMT (by HORKANS at YKTVMV)
Re: H diffusion in Pd

I did some work years ago on the electrochemistry in the hydrogen adsorption/absorption region of Pd. The electrodes were evaporated thin films, and there was a very large dependence on film thickness and on grain size. I concluded that for H in Pd (a rather unusual system) the lattice diffusion rate of the H was greater than the grain-boundary diffusion rate. Thus, movement of H into and out of the interior of the Pd occurred much faster in materials not having many grain boundaries (i.e. grain boundaries acted as impediments to diffusion).

It would seem to me that the grain structure of the Pd could influence the "RTF" results. Maybe the high-temperature pretreatment could anneal the Pd into a larger-grained structure, allowing faster access of the H to the interior of the sample and thus shortening the amount of time necessary to start the energy-producing reaction. The distribution of the H in the Pd sample could be important. The differences in structure and geometry of the various Pd electrodes used might certainly explain some of the differences in results for the various groups exploring this system.

Jean Horkans

----- FUSION FORUM appended at 18:54:48 on 89/04/11 GMT (by RDHARPER at SJEVM5)
Subject: Texas A&M Fusion
Ref: Append at 16:08:20 on 89/04/11 GMT (by MCINNIS at AUSVM9)

At the risk of rushing the point of the experiments, we need better than a 1.6 gain in energy, as the efficiency of conversion from heat to useable power will be around 50%.

But more power to 'em!

Regards,
RDH
11:51:49 PST * * * 04/11/89

----- FUSION FORUM appended at 19:11:43 on 89/04/11 GMT (by PONTIUS at BTVVMLW)
Subj: Cathodes (not truly expert, but former physics major)

It seems necessary to ask if anyone has analyzed the cathode after the 'fusion reaction' has taken place. Looking in my handy CRC book I see than Palladium has a neutron absorption cross section in the range of 4 barns or so.

The factor that may make some difference is that if neutrons are being generated by some sort of fusion reaction, THEY ARE BEING GENERATED AT SPECIFIC SITES INSIDE THE LATTICE rather than from outside the lattice with no particular positional or directional orientation. This may make the probability for fusion-generated neutrons being absorbed by Palladium significantly different from the standard figure. Whether that would be larger or smaller I don't know.

Palladium has several isotopes of various half lives, many of them stable. There is a good chance of reaching several isotopes (my CRC is not with me now, sadly) that beta decay into Silver with half lives in the range of seconds to hours, any of which are reasonable for the experimental time frames.

Silver has a typical cross section about 10x that of Palladium, and has various isotopes and half lives, some of which decay into Cadmium. Cadmium has an incredible neutron cross section. After looking at the CRC value I understand why they use it for damping rods in nuclear fission reactors.

The room temperature fusion reaction may well 'burn' Palladium as well as Deuterium. (however quickly or slowly is another question.)

The upshot of all this is that:
1: Cathode shape will definitely have an effect on nuclear reactions.
2: Having the electrode change from Palladium to Silver and/or Cadmium could well have an effect on the lattice and hence the Deuterium absorption capability and fusion reaction rate.
3: If the reaction 'burns' Palladium too quickly then any sort of practical application of this brand of room temperature fusion looks pretty grim, as Palladium is not in abundant supply.

Dale Pontius

----- FUSION FORUM appended at 21:44:26 on 89/04/11 GMT (by PORCHED at ATLVM2)
Re: One basic question

As a total amateur, I should not be writing here, but one question keeps coming to mind.
In considering the energy out being 1.6 times (or greater) the energy in, is everyone taking into consideration the amount of energy it took to "ferment", for weeks, the Palladium rod with current?

Ed Porcheddu (ATLVM2 PORCHED)

----- FUSION FORUM appended at 11:50:30 on 89/04/12 GMT (by AMANTE at PKSMRVM)
Subj: One (more) basic question
Ref: Append at 21:44:26 on 89/04/11 GMT by PORCHED at ATLVM2

Also being an amateur, I was wondering the same thing. If the energy being seen is not from fusion, could it just be the release of the energy stored for weeks by the low voltage applied to the palladium rods?

Also being an amateur, I thought electricity was directly producible from a fusion reaction. I remember something about just winding a coil around the reaction vessel and an electric current would be produced. If this is really true, then the efficiency of conversion from heat would not be a problem. No?

Walt

----- FUSION FORUM appended at 13:21:29 on 89/04/12 GMT (by MUELLER at AUSVM6)
Subject: One (more) basic question, direct energy conversion
Ref: Append at 11:50:30 on 89/04/12 GMT (by AMANTE at PKSMRVM)

It's called MHD,(Magneto Hydro Dynamics,) and only works with high temp. plasma's. Channeling plasma, (highly energized particles,) thru a magnetic coil allows you to pull off a current.
Analogous to a transformer in operation, and if a super conducting coil is used, near 100% direct conversion is possible.

Jerry

----- FUSION FORUM appended at 13:55:58 on 89/04/12 GMT (by BISON at MSNVM3) --
Subj: One (more) basic question
Ref: Append at 11:50:30 on 89/04/12 GMT (by AMANTE at PKSMRVM)

Re: Energy out. I also was looking for this. At least one report from one of the groups explicitly stated that the energy out was more than the total energy - including everything from the start of the 'charging' of the palladium.

Re: electricity. To get usable electricity, you must get electrons moving thru a wire. A fusion reaction produces neutrons and radiation which can be converted to thermal energy. Neither of these will directly create electrons, or a magnetic field which can be used to get already present electrons moving in a wire.

Re: Today's presentation at ACS in Dallas. The local all news radio said Pons would present his report at 12:15 and there will be a press conference at 3:00. Neither will be broadcast live. However they will broadcast live a sports press conference at that time. Make up your own comment!

Bob Bison

----- FUSION FORUM appended at 13:56:26 on 89/04/12 GMT (by SOREFF at FSHVMFK1)
If the room temperature fusion reactions are real, it could be quite interesting to find out both the temperature and pressure dependence of the reaction rate, both for steady state and transient conditions. As things currently stand, the only mechanism that would clearly reduce the reaction rate at elevated temperatures is desorbtion of the deuterium from the palladium catalyst. This will tend to stabilize the steady state reaction rate but it may not have much effect on fast transients. If a resonance mechanism is involved, that may set an upper limit on the temperature at which the reaction can occur, but no one knows if this is true or not. If we take the simplest guess and assume that the reaction acts like we'd expect a tunnelling process to act, but enhanced by many orders of magnitude for an unknown reason, then we'd expect exponential increases in the reaction rate due to either temperature or pressure. There isn't any obvious limit to fast thermal runaway except fusion of all the deuterium or disassembly of the experiment. If this speculation happens to be true then
a) Researchers may want to do experiments on as small a scale as possible, in a setup designed to minimize thermal and pressure transients
b) If this technology winds up in use for power production, one of the usual arguments for the inherent safety of fusion (that a magnetically confined reactor automatically shuts down if anything goes wrong) will not apply, and the same safeguards that are needed to avoid thermal transients in fast neutron fission reactors may be needed, though we still avoid the problems with residual heat from beta decay of fission products after the reaction has been shut down.
c) Fission-free fusion explosives may be possible by deliberately triggering runaway. For all I know, simply putting deuterium-saturated palladium in the same sort of implosion arrangement used for fission explosives might work. One would certainly expect many orders of magnitude increase in reaction rate from raising the factor-of-two or so compression that this would produce if the reaction is a tunnelling process. -Jeffrey Soreff

----- FUSION FORUM appended at 14:05:54 on 89/04/12 GMT (by KESSELMN at KGNVMF)
..... FUSION FORUM modified at 21:45:20 on 89/04/12 GMT (by KESSELMN at KGNVMF)
<Amateur comment evaporated>

----- FUSION FORUM appended at 14:29:41 on 89/04/12 GMT (by ACW at YKTVMV) ----
Subject: One (more) basic question

Ref: Append at 11:50:30 on 89/04/12 GMT (by AMANTE at PKSMRVM)

one of the early appends mentioned that P&F included the power needed to charge the Pd in their net power calculations...

no new reports from the Yorktown or Almaden groups on confirmation?? (just a dig, Ga. Tech is my alma mater!)

Alan C. Warren

----- FUSION FORUM appended at 14:33:11 on 89/04/12 GMT (by MIKCLRK at HVTVM2)
Subject: 'the inherient safty of fusion'
Ref: Append at 13:56:26 on 89/04/12 GMT (by SOREFF at FSHVMFK1)

What happens when the magnetic bottle fails ?

(Please answer in the TECHNOL FORUM if you want to carry on).

Mik Clarke

----- FUSION FORUM appended at 17:13:53 on 89/04/12 GMT (by HALLDJ at ENDVMTKL)
Subject: "It's not what you make, it's what you keep"
Ref: Append at 14:29:41 on 89/04/12 GMT (by ACW at YKTVMV)

The temperature the process can be made to operate at is critical to it's utility. "Room temperature" fusion(ie at 25 Deg C) would be practically useless for electricity generation. For such an application thermal efficiency is figured as 1-(Qlow/Qhigh) which is equal to 1-(Tlow/Thigh) (This is for the carnot cycle which is an idealized cycle that approximates real generation plant cycles). Qhigh in our case is the thermal energy supplied by the RTF source at a temp. Thigh. Qlow is the heat rejected to the river nearby or the cooling tower at Tlow. Environmentally, low thermal efficiency would mean that for the thermal energy produced by RTF relatively little work (hence little electricity) is produced a lot of heat gets rejected. You get high thermal efficiency by providing a large temperature difference. Since Tlow is pretty much fixed, you would want as high a Thigh as possible.

Some examples:

Assume Tlow is 27 deg C (300 K)

Thigh K	Thermal Efficiency
====	====
473	37% (Boiling water at 1 Bar)
800	62% (pressurized water. Fission reactor?)
1846	84% (Melting point of Pd, Theoretical RTF upper limit?) (coincidentally, not a bad approx for a fuel fired plant)

This isn't to say that RTF isn't exciting and very significant if true. I am saying that part of scaling the process up will be to raise the temperature to useful levels. This may be one reason Pons estimates so long for commercialization.

Some engineering ramifications might be that the RTF reactor would require pressure vessels to raise the boiling point of heavy water to useful levels. To conserve the "witches brew" it may mean a secondary heat exchanger loop. The good news is that a lot of this technology has been developed for Fission reactors.

Several years ago the hot topic was Magneto-Hydrodynamics as the generation cycle. It's attraction was it used a very high Thigh (5000 deg c as I remember=> Th. Eff.=95%). It was based on fast moving streams of charged particles (ie plasma). The plasma eventually could have been produced by a physical confinement fusion reactor. This may be why Fleischman (sp?) indicated existing fusion research should continue.

D.J.Hall

----- FUSION FORUM appended at 20:35:09 on 89/04/12 GMT (by RRAUSCH at DALVMIC3)
Re: Back on Track...

One of the real attractions (and therefore, to me the real benefit) of this forum is its founding committment to 'Expert Only' input. Let's remind the 'amateurs' and physics majors to take their electric coil and magnetic bottle conjecture to TECHNOL or NEWSTALK and not clutter this spectacular scientific discussion with non-expert stuff. It may bother the real scientists in IBM enough to find some other vehicle. That would be a loss for all of us...

Regards, Ron Rauscher

----- FUSION FORUM appended at 12:20:34 on 89/04/13 GMT (by THATCH at PK705VMA)
Subject: USSR Verification

NPR reported this morning that Russian scientists had done 20 fusion experiments and 'verified' the U of U / BYU experiments. No details were given.

R. Jonathan Thatcher

----- FUSION FORUM appended at 13:04:22 on 89/04/13 GMT (by MACNAIR at RHQVM08)
Re: More news

There are 3 articles in todays New York Times (page D21) related to Cold Fusion. Regretably, they gave little new insights, despite a significant article on Dr. Pons first scientific meeting since his news conference announcing his results. The only things I found of significance were:

Dr Pons stated that the heavy water contained a strong enough solution of Lithium Hydroxide to "etch glass". Thus, I would assume that anyone trying to duplicate the experiment use a lot of Lithium Hydroxide.

Soviet scientists (Runar Kuzmin at Moscow University) claim to have duplicated the experiment, finding it "surprisingly easy". The article claimed they had "performed 20 experiments which had proved the phenomenon". No details on what they actually did or what the "20 experiments" were.

MIT has applied for patents based on theorectical work by Dr. Hagelstein, an associate professor in MIT's electrical engineering and computer science department (????). Dr. Hagelstein is created with invention of the X-ray laser in this article (?????). He claimed to have submitted 4 papers describing his work, but seems reticint to discuss it prior to acceptance of the papers (patent applications might also make him reluctant).

Regretably, the articles contain large amounts of drivel about chemists chortling about their major coup in what seems to be the biggest discovery in physics, and a lot of space to a very defensive sounding head of fusion research at Princeton ("No physicists are going to do work on this until its proved ... We're just about there with two convential approaches to fusion (?????)). I wish they had reported on what Pons and others had to say about the experiment.

Jim Mac Nair

----- FUSION FORUM appended at 14:26:01 on 89/04/13 GMT (by FISCHTHS at GBGVM7)
Subject: More news
Ref: Append at 13:04:22 on 89/04/13 GMT (by MACNAIR at RHQVM08)

According to the Washington Post, the Princeton physicist wanted to see a control experiment, where light water was used in place of heavy water, to see if they get the same results.

The Post went on to say that some of the previously skeptical physicists are more optimistic now that several parts of the experiment have been duplicated, some by well-respected physicists, though everyone still wants to see one group reproduce all the results.

Scott Fischthal

----- FUSION FORUM appended at 14:59:35 on 89/04/13 GMT (by MIKCLRK at HVTVM2)
Subject: Thoughts about the reactions.
Ref: Append at 17:09:41 on 89/04/11 GMT (by MIKCLRK at HVTVM2)
Warning: Long append.

Appending an off line discussion with Glenn Knickerbocker about the referenced append - maybe it'll be of use to some-one else.

Glenn Knickerbocker

Interesting observation! But why should the grain boundary interstices be *less* attractive for hydrogen? Most things move *more* easily through grain boundaries, right? Or is hydrogen driven into the lattice more easily than other things because it's light?

Me

I was thinking that the normal interstatial sites are as far away from the palladium nucli as you can get in the lattice, and surrounded by -ve charge - the sort of place a +ve ly charge duterium atom might like to go. At the boundries you'll only have partial sites, which are closer to the nuclus of one of the atoms bordering it, and *may* have less effective -ve charge on them than the normal interstatial sites, thus being less attractive to the duterium (they'll move into them and carry on through into the palladium on the far side, or keep jumping along the boundry as long as some-where with more -ve charge attracts them (maybe)).

Glenn Knickerbocker

But you'll get the highest *transient* densities where there's the most mobility. Since the reaction rate decreases exponentially with distance, what's of interest is the nearest approach, not the average distance.

Notice, though, that there's an append just below yours mentioning research that shows that hydrogen moves faster through the palladium lattice than through the grain boundaries!

Me

The average approach at the boundries (if I remember the figuers ok) will be about .35 A (the side of a unit cell being .7 A), but there are bound to be some split unevenly on the boundry (a 75/25 split gives a closest approach of < .2 A !) - you may even get 'double' sites where they're very close.

With the flow rates you might find that the number of duterium atoms would build up on one side of a grain boundry, but have a much lower concentration on the far side - until both grains were 'full' the duterium atoms wouldn't hang around on the boundries (in the points of nearest approach) as they'd be free to move on into the 'empty' crystal.

It would be nice to know if the slower speed was along or accross the grain boundries (both I would expect - to a duterium nucli, palladium is mostly space, but slightly fuller along the grain boundries where there is a change in the lattices...)

```
      . . . . . . .   Grain 1
      . . . . . . .
      . . . . . . .  █Apparent high density region on the boundry.
       . . . . . . . /
       . . . . . . .
       . . . . . . .  Grain 2       ( not a very good diagram really ).
```

Would this mean that palladium made up of large crystals, but with some boundries is best ? (and that mono-crstalline palladium wouldn't work ?).

- And now some new thoughts...

This would give rise to a very interesting diffusion pattern for the duterium through the grains, with the duterium only being allowed to build up along the grain boundries when ALL the grains in the palladium were nearing saturation.

Again, if some-one can tell me this is junk I'll stop.

Mik Clarke

1 – The two discoveries | M. Fleischmann 10
B. Czirr – 12^05
S. Jones –

2 – Questions raised by the experiments | R.L. GARWIN

3 – Status of new experiments
MM. BROER – AT&T, Bell Labs,
R. HAHN – BNL
J. Ziegler – IBM

4 – What kind of critical expts can be done? L. Ponomarev & S.S. Gerstein

5 – Non-fusion sources of heat T. HO

6 – Theoretical [Calculations KOONIN BENEDEK
WILKINSON MAIANI [What kind of F.R. can provide energy without particles & gamma rays.

8 – Ancient History of Pd Catalyzed fusion. H. GERISCHER.

7 – New proposals

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Il successo del seminario di Erice: l'atto della riconciliazione è

Abbraccio tra i principi d

L'esperimento Jones sarà ripetuto nel labor

Fleischmann e il «rivale» hanno espresso l'intenzione di collaborare pe chi: «Propongo di trasformare la base di Comiso in un pacifico punto d'i

DAL NOSTRO INVIATO

ERICE (*Trapani*) — Al centro Majorana di Erice, sotto l'accurata regia del professor Antonino Zichichi, abbiamo assistito a un altro atto della scienza-spettacolo. Soggetto: la fusione nucleare fredda. Interpreti: Martin fleischmann e Steven Jones, i due scienziati dello Stato americano dello Utah che si contendono il primato della scoperta.

A un cenno dell'infaticabile presidente del centro Majorana i rivali si sono stretti la mano e quasi abbracciati. Poi, mentre Zichichi li teneva stretti, braccia sulle spalle, i due hanno pronunciato l'atto di riconciliazione davanti alle telecamere: «Effettivamente si potrebbe lavorare insieme per ripetere l'esperimento». «Già, sarebbe bello collaborare per approfondire queste ricerche».

Ma Zichichi non si è accontentato di impegni vaghi e ha tagliato corto: «Ma sì, mi hanno promesso che lavoreranno insieme agli studi sulla fusione fredda qui ai seminari di Erice. C'è di più. Ho già proposto che la base missilistica di Comiso venga trasformata in pacifico laboratorio internazionale per gli studi di vario tipo sulla fusione nucleare. Fleischmann, Jones, e gli altri che si occupano di questi studi mi aiuteranno a portare avanti il progetto».

Lieto fine dopo un prologo piuttosto difficile. Martedì sera, all'arrivo nella cittadina siciliana, Fleischmann e Jones sembravano intenzionati a non scambiarsi nemmeno un saluto. Zichichi e il presidente della Regione siciliana Nicolosi avevano dovuto sottoporsi a una estenuante doppia cena. Primo turno con Jones, il fisico mormone di Provo arrivato con un seguito di cinque figli e moglie incinta.

Congedato il primo ospite Zichichi e Nicolosi, senza nemmeno alzarsi da tavola, hanno atteso l'arrivo di Fleischmann e si sono lanciati a denti stretti in una seconda cena ufficiale. Per la seconda volta i camerieri hanno servito pastasciutta con polpette di carne e aragosta, mentre al tavolo accanto un chimico faceva il computo delle calorie complessivamente ingurgitate.

Ma non è stata fatica inutile. Il seminario di Erice si è risolto in un autentico successo. Rasserenati per la pace fatta, i due scienziati dello Utah hanno dato il meglio di sé stessi, illustrando, con ricchezza di particolari, i meccanismi dei rispettivi esperimenti a circa duecento specialisti venuti da ogni parte del mondo per ascoltarli.

Per il dibattito Zichichi ha richiamato esperti di primo piano come gli americani Garwin e Wilkinson, gli italiani Maiani e Palumbo, il cinese Ho, il russo Ponomarev. La valutazione delle conseguenze geo-cosmologiche della fusione fredda è stata affidata ai professori Barberi e Boschi.

«La fusione fredda esiste. Sulla realtà del fenomeno io, come molti altri fisici, non abbiamo più dubbi», ha potuto concludere il professor Zichichi. Restano da chiarire le discrepanze fra i due esperimenti. Quello di Jones, che la maggior parte dei fisici giudica il più coerente, perché il processo di fusione è accompagnato da un flusso di particelle in buon accordo con le teorie, esce dal confronto come il più credibile, anche se il meno adatto ad eventuali applicazioni.

Zichichi ha annunciato che questo esperimento sarà replicato nel laboratorio del Gran Sasso sotto la direzione dei fisici bolognesi Bertin e Vitale.

L'esperimento Fleischmann lascia ancora perplessi perchè l'enorme quantità di calore che sviluppa non si accorda con la modesta emissione di particelle. Se, tuttavia, dovesse anche questo ottenere conferme sperimentali, è ritenuto quello più adatto ad applicazioni commerciali.

Franco Foresta Martin

Il Tar contro Ferri Nei giorni festivi nessun camion sulle autostrade

ROMA — Da domenica 16 aprile i Tir non potranno circolare nei giorni festivi sulle autostrade e sulle strade statali a meno che non siano autorizzati dal prefetto.

La circolare del 19-12-1988 con la quale il ministro dei Trasporti Ferri aveva autorizzato la circolazione sulle autostrade ai mezzi pesanti adibiti al trasporto di merci deperibili è stata sospesa dal Tar del Lazio dopo un ricorso del Coordinamento delle associazioni degli utenti. Al ricorso si erano opposti i produttori di generi di prima necessità.

Eletto a sorpresa l'illustre clinico C

Caso Mangiagall

MILANO — La «Mangiagalli» ha un nuovo presidente del consiglio di amministrazione: al posto di Angelo Craveri, cui, nonostante l'opposizione della Dc, è stato revocato l'incarico, è stato nominato Carlo Zanussi, consigliere di area socialista. La sostituzione è avvenuta ieri, durante la seduta del consiglio di amministrazione degli Istituti clinici di perfezionamento, (da cui dipende anche la Mangiagalli), che era stato convocato appositamente.

La riunione non ha richiesto molto tempo: non appena si è vista chiara la tendenza dei consiglieri di revocare l'incarico al presidente in carica, Craveri si è allontanato e non ha voluto partecipare al voto. Come si è detto, è stato nominato nuovo presidente Carlo Zanussi, figura di grande prestigio in cam
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Anche Mosca conferma la scoperta

MOSCA — Anche gli scienziati russi avrebbero sperimentato la fusione fredda. Sotto la guida del professor Runar Kuzmin, ricercatori del laboratorio di fisica delle materie solide dell'Università di Mosca hanno confermato l'esperimento degli americani. La notizia è dell'agenzia Tass.

Già nel 1981-'82 il professor Kuzmin e il suo assistente, Vladimir Vjsotskj avevano pubblicato un lavoro teorico che mostrava, in linea di principio, la possibilità di ottenere la fusione a temperatura ambiente.

«Quando lo scorso maggio ho saputo di esperimenti fatti all'Università dello Utah, ho deciso di ripeterli immediatamente» ha detto lo scienziato.

Nelle ultime due settimane Runar Kuzmin con i ricercatori Boris Shvilkin e Eugheni Sakharov hanno compiuto circa venti prove che hanno confermato la possibilità di fusione nucleare a bassa temperatura.

«Gli esperimenti stessi sono sorprendentemente semplici» ha detto lo scienziato e, spiegandone le modalità, ha sostenuto che è stato anche registrato il rilascio di neutroni veloci con l'aiuto di un detector.

L'incontro fra gli scienziati americani Steven Jones e Martin Fleischmann al centro «Majorana» di Erice. E' stata una sorta di rappacificazione dopo le polemiche esplose per l'annuncio di Fleischmann e Pons del riuscito esperimento di fusione fredda (Foto Ansa)

rlo Zanussi, ma la nomina potrebbe essere impugnata per illegittimità

li: cacciato il presidente Craveri

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glio comunale. In questo modo a nulla è servita l'opera di mediazione dei due assessori regionali Mario Fappani e Ugo Finetti che, su incarico del presidente Giuseppe Giovenzana, si erano impegnati a trovare una via d'uscita che riportasse la Mangiagalli in un clima di serenità senza ulteriori traumi.

Ora si attendono le mosse della Dc in opposizione alla sostituzione di Craveri: è possibile che la delibera di nomina del neopresidente venga impugnata per illegittimità dinanzi al Coreco (Comitato regionale di controllo), e allora la bagarre potrebbe tornare.

Nel frattempo continua l'azione dei sostituti procuratori della Repubblica Daniela Borgonovo e Piero Forni incaricati dell'inchiesta sul «caso Mangiagalli».

Si è saputo che ormai sono due i procedimenti avviati: oltre a quello relativo all'aborto terapeutico su una donna al quinto mese di gravidanza del 28 dicembre scorso, i due magistrati devono anche occuparsi delle cartelle cliniche «sospette» inviate a palazzo di giustizia dal ministro Carlo Donat Cattin.

Il fascicolo per questo secondo caso è già stato ufficialmente aperto, ma non ha ancora alcun inquisito, e comunque resterà separato da quello che vede imputati i due ginecologi autori dell'intervento sotto giudizio, Francesco Dambrosio e Bruno Bramati, la biologa Leda Dal Prà e il medico di fiducia della donna, tutti indiziati di aver violato la legge 194.

Pure imputati nello stesso procedimento sono Leandro Aletti e Luigi Frigerio, i due ginecologi che con le loro dichiarazioni al quotidiano cattolico «Avvenire» avevano fatto esplodere lo scandalo. I due sono stati convocati dai giudici domani: dovranno rispondere di violazione del segreto professionale in quanto, secondo l'accusa avanzata dallo stesso consiglio di amministrazione della «Mangiagalli», hanno reso pubblico il dramma di una donna ricoverata per aborto e la cui identificazione è risultata alla fine inevitabile.

Per quanto riguarda gli altri inquisiti, invece, i magisrati devono aspettare l'esito della perizia disposta per verificare la fondatezza della diagnosi in base alla quale la donna al centro della vicenda ha potuto essere sottoposta a interruzione di gravidanza.

Augusto Pozzoli

Caso Serena
Vassalli ai giudici
«Io non interferisco sono altri a farlo»

TORINO — Continua la violenta polemica tra i giudici torinesi e il ministro Vassalli sul caso della piccola Serena. Martedì i magistrati avevano accusato l'esponente politico di «inammissibile interferenza nell'attività giurisdizionale» e Vassalli aveva replicato dichiarando di non stupirsi delle accuse «in quanto provengono da circoli che da tempo denigrano il ministro della Giustizia e il Governo».

Ieri il ministro guardasigilli è tornato all'attacco.

«L'interferenza — ha detto — la fanno queste associazioni, sezioni, sottosezioni che, mentre è ancora pendente un ricorso di cui debbono giudicare alcuni magistrati, esaltano quello che è stato fatto dagli altri: sono loro che fanno una chiara interferenza perché legano i loro colleghi manifestamente a questa solidarietà nelle decisioni già adottate».

«Quindi — ha aggiunto — è veramente inconcepibile che compiendo un grave atto di interferenza lo si imputi a chi non lo ha fatto».

La reazione alle parole di Vassalli non si è fatta attendere.

Ha dichiarato il presidente della sezione Piemonte e Valle d'Aosta dell'Associazione nazionale magistrati, Francesco Marzachì: «Mi rammarico e mi addolora che vi siano "circoli" da tempo dediti alla denigrazione dell'attuale ministero della Giustizia, ma noi non ne facciamo parte. La nostra è stata una protesta assolutamente civile, contenuta in un documento dai toni "soft" e dagli aggettivi riguardosi.

«Evidentemente — ha aggiunto Marzachì — il ministro non si riferiva a Torino accennando a quei circoli denigratori. Noi ci siamo limitati a segnalare una situazione di disagio tra i giudici».

Erice. Dopo le polemiche sulla primogenitura della
ieri Martin Fleischmann e Steven Jones si sono stretti

Tregua fra i duellanti

L'esperimento sarà ripetuto anche da scienziati italiani nel laboratorio sotterraneo del Gran Sasso

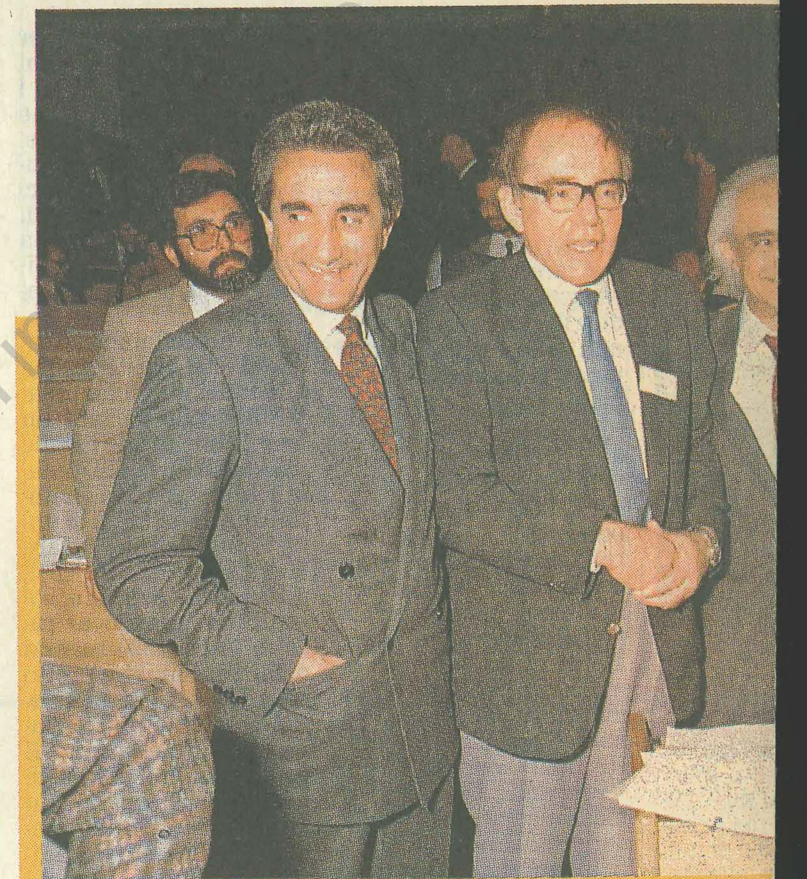
Dal nostro inviato

ERICE — Rivali? Macché. Nemici? Nemmeno. In lotta testa a testa per finanziamenti a molti zeri, per la gloria di un Nobel? Neanche a parlarne. Se c'era qualcuno che dal faccia a faccia tra i due scienziati che si contendono la primogenitura della fusione nucleare «fredda» si aspettava scaturissero i neutroni della polemica, balenassero i raggi gamma delle accuse velenose, è rimasto deluso. Sotto le austere volte del monastero di San Domenico di Erice, ormai consacrate ai grandi abbracci della scienza internazionale, anche Martin Fleischmann e Steven Earl Jones si sono stretti la mano, hanno sorriso insieme davanti alle telecamere, hanno posato l'uno accanto all'altro di fronte a fotografi a caccia di scoop, si sono sottoposti all'assalto di fisici, chimici, geologi, ingegneri, giornalisti, tutti desiderosi di scoprire se i duellanti dello Utah sono gli Enrico Fermi del Duemila o soltanto alchimisti abbagliati da qualche luccichio dorato.

E così, dimenticate le

stiere di scienziato. Sessantadue anni, elettrochimico, cecoslovacco di nascita, inglese di adozione, Martin Fleischmann è un europeo vulcanico, esuberante, amante della risata a piena gola e del whisky (lui stesso ha ammesso che un ruolo importante nello storico esperimento di Salt Lake City lo ha avuto, oltre all'ormai mitica sbarretta di palladio, anche una bottiglia di bourbon «Jack Daniel»). Apparso sulla scena del centro Majorana con i pochi capelli scompigliati, la giacca aperta e la camicia azzurra, evocava figure da pub londinese. E l'improvvisa notorietà, più che turbarlo, pare di capire che lo galvanizza.

Di tutt'altra pasta è fatto Steven Jones, 40 anni, fisico per scelta e mormone per vocazione (vent'anni fa ha fatto il missionario in Belgio e in Francia): longilineo, faccia da timido primo della classe beffato dal colpo di genio di quello dell'ultimo banco, capelli che dal biondo sfumano in grigio, occhialini, camicia candida e cravatta regimental, è arrivato a Erice con tre dei suoi cinque



Lo dicono alcuni esperti

Una teoria formulata da tre studiosi oltre quarant'anni fa?

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fusione nucleare «fredda», la mano al centro Majorana

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L'agenzia di stampa sovietica ri-

PANTELLERIA

Indagine su presunte spie

PANTELLERIA — I carabinieri stanno attuando accurati servizi di vigilanza nelle zone militari dell'isola di Pantelleria, per una possibile presenza di spie o di sabotatori. Nulla è però emerso finora. I servizi sono coordinati dal capitano Pierfranco Fiaccalvieri comandante della compagnia dell'Arma a Marsala dalla quale dipendono i carabinieri di Pantelleria. Gli impianti militari dell'isola, considerati importanti nello scacchiere difensivo del Mediterraneo, sono stati visitati dal comandante della regione militare della Sicilia gen. di corpo d'armata Piero Monsutti.

TURISMO ED ECOLOGIA

Convegno sulle isole minori

PALERMO — Un convegno sulla «Gestione ecologica del territorio, presupposto di sviluppo turistico delle isole minori della Sicilia», si terrà domani e sabato alla Fiera del Mediterraneo, nell'ambito della Medivacanze '89. Al simposio, organizzato dall'Azienda provinciale per il turismo di Palermo e dall'Associazione ecologica Marevivo, parteciperanno numerosi esperti italiani e stranieri del settore. La giornata del sabato sarà dedicata al tema: «La gestione dei parchi e delle riserve marine quale occasione per un diverso progetto di sviluppo».

TECNICI AFRO-ASIATICI

Visitato l'Enimont di Priolo

SIRACUSA — Cinquanta funzionari dei paesi afro-asiatici, che stanno frequentando un corso annuale presso la scuola di pubblica amministrazione di Reggio Calabria, hanno visitato il polo petrolchimico dell'Enimont di Priolo. Gli ospiti stranieri, accompagnati da Giuseppe Liotta, direttore della scuola, si sono incontrati con i direttori del complesso petrolchimico con i quali hanno avuto uno scambio di informazioni. Prima di Siracusa, i funzionari afro-asiatici, avevano visitato il centro Ettore Maiorana di Erice e la Valle dei Templi di Agrigento.

SIRACUSA

Puerpera morì, medici a giudizio

SIRACUSA — Due medici del reparto graviliti a distanza e le invettive reciproche (Jones che accusa Fleischmann e il suo collega Stanley Pons di avere tradito il patto per l'annuncio simultaneo delle scoperte, Fleischmann che replica: «Spero che la verità non venga fuori: ci rimetterebbe solo Jones»), i due scienziati si sono premurati a inneggiare all'amicizia, alla collaborazione scientifica, alla cooperazione in nome dei destini dell'umanità. Il tutto sotto lo sguardo sornione ed euforico di Antonino Zichichi, nella triplice veste di fisico nucleare, manager della scienza internazionale e uomo di pace.

Le cronache delle prossime settimane ci diranno se è veramente pace, oppure se è soltanto una tregua imposta dalla diplomazia di Zichichi. Quel che è sicuro, comunque, è che i due «padri» della fusione a freddo, pur operando a una sessantina di chilometri l'uno dall'altro, sono i prototipi di due opposti modi di intendere il me-

bambini e con la moglie che ne attende un sesto. Più che un uomo da scienza-spettacolo, sembra un predicatore in cerca di proseliti.

Potranno un giorno lavorare insieme, Fleischmann e Jones, superando la rivalità delle rispettive università dirimpettaie (la laica Utah University di Salt Lake City e la mormone Brigham Young University di Provo) a caccia dei maxifinanziamenti statali? Rubando per qualche attimo la parola nel corso di una breve conferenza stampa dominata dalla verve di Zichichi, Fleischmann è rimasto sul vago: «Abbiamo progettato tantissimi esperimenti — ha detto — e per condurli sia io che il professor Jones abbiamo bisogno della collaborazione degli scienziati di tutto il mondo». E capita l'antifona, Jones si è scoperto anche lui diplomatico: «Per noi è naturale collaborare con tanti colleghi. A me potrebbe fare piacere operare insieme

faccia a faccia. Al termine della prima sessione il vincitore morale sembrava Jones.

I lavori erano stati introdotti dal direttore del Centro, professor Antonino Zichichi a cui va il merito di aver posto i due grandi gomito a gomito tra loro con la comunità scientifica internazionale. «Il forum è stato organizzato in soli otto giorni», ha dichiarato Zichichi. Le otto sessioni di lavori sono state seguite interamente dal presidente della Regione Siciliana, On. Rino Nicolosi; nel suo saluto ha sottolineato che la Sicilia, con il Centro Ettore Majorana, costituisce «un catalizzatore per la scienza internazionale e dà il proprio contributo alla pace».

L'on. Nicolosi ha confermato l'impegno del governo regionale per le realizzazioni scientifiche nell'isola, con particolare riferimento al Laboratorio mondiale ed al premio «Erice: scienza per la pace».

La mattinata è stata poi interamente dedicata alle relazioni di Fleischmann e Jones che hanno risposto ai quesiti

proposto di variare la pressione, perché secondo le teorie, i loro due risultati potrebbero coincidere. Ma Jones, dall'alto della pedana ha negato recisamente.

Nel pomeriggio il confronto scientifico si è fatto ancora più interessante perché è emerso che la teoria della fusione a freddo era stata già formulata nel 1947 da Lattes, Occhialini e Powell. Andrej Sakharov la aveva ripresa l'anno successivo e nel 1954 Zeldovich ne aveva dato una stesura più dettagliata. Assieme a Sergiej Gershtein, allievo del fisico Landau, premio Nobel sovietico per la fisica del 1962, Zaldovich aveva pubblicato uno scritto nell'agosto del 1960 proprio sulla fusione a freddo.

Certo i tempi non erano ancora maturi per realizzarla, ma essa era già stata teorizzata in questi termini: ponendo un chilo di deuterio sotto la pressione di 600x10 alla sesta atmosfere, si otteneva, secondo i calcoli, una reazione ogni minuto. Il valore di Jones, va ricordato, è di 0,4 fusioni al secondo.

Bloccati venticinque miliardi

Sogesi, impugnata la legge della Regione

PALERMO — Il commissario dello Stato presso la Regione siciliana ha impugnato la legge in favore della Sogesi approvata la settimana scorsa all'Assemblea. Il dott. Antonino Prestipino ha censurato l'articolo con il quale vengono assegnati alla società 25 miliardi per assicurare il buon andamento delle gestioni esattoriali in Sicilia.

A parere del commissario dello Stato, che ha mostrato di badare più alla sostanza che non alla forma del provvedimento, l'intervento finanziario della Regione va considerato come un modo surrettizio di incremento dell'agio esattoriale. Muovendo da questa premessa il commissario dello Stato eccepisce che la Regione, in base allo Statuto, ha poteri limitati in materia finanziaria e di conseguenza non può stabilire una misura dell'agio diversa da quella vigente in campo nazionale.

Il disegno di legge approvato dall'Assemblea ha avuto un lungo e tormentato iter parlamentare connotato dalle numerose audizioni tenutesi in commissione Finanze. Alla fine, sembrava che la vicenda potesse sbloccarsi grazie ad un intervento della Regione di 42 miliardi, 17 dei quali per i maggiori oneri sostenuti dalla Sogesi dopo la omogeneizzazione del trattamento economico del personale e 25 per le gestioni esattoriali. Ma quest'ultima norma è incappata nella censura del commissario dello Stato, come era stato previsto da diversi gruppi dell'opposizione e da ultimo dal Pli.

Il commissario dello Stato ha invece riconosciuto pienamente legittime le altre due leggi approvate dall'Assemblea. Sono quella per la metanizzazione e quella che eleva a 40 anni il limite di età per la partecipazione ai concorsi.

P.F.

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Il Segretario Generale
Dr. A. Zappalà
Pr

ferisce che lo scienziato ha attuato il suo esperimento in acqua «pesante», «nella quale molecole di atomi di idrogeno sono state sostituite con quelle di deuterio». «Applicando una corrente elettrica che misura fino a 0,00003 ampere, i ricercatori hanno osservato che l'acqua si riscalda fino a raggiungere il punto di ebollizione ed a liberare neutroni veloci», scrive l'agenzia.

Il breve dispaccio conclude con la seguente dichiarazione dello scienziato: «Abbiamo registrato una liberazione di neutroni che era da tre a cinque volte più ampia di quella di fondo. La liberazione di neutroni consente di affermare con certezza che vi è stata la reazione di una fusione nucleare».

Intanto mentre cresce negli Stati Uniti la febbre da «fusione» dopo l'esperimento di Stanley Pons e Martin Fleishmann all'università dello Utah, in Italia il parlamento si appresta a varare una indagine conoscitiva che studierà in tempi brevi le caratteristiche e le implicazioni tecniche dei risultati americani, nonché la fattibilità e le prospettive che in tal campo si aprono anche per il nostro paese.

Domani alle ore 10,30, presso l'aula della presidenza della facoltà di Ingegneria in viale delle Scienze a Palermo il professore Martin Fleischmann illustrerà i suoi esperimenti sulla fusione fredda.

mancanza di rivelazioni nuove e dall'esibizione di diapositive che mostravano attrezzature da «piccolo chimico», i fisici presenti al forum sono sembrati parteggiare, più che per gli elettrochimici di Salt Lake City, per il fisico mormone: «Quello di Jones è un esperiemnto convincente, si capisce tutto», ha detto Zichichi. Forse anche per questo due scienziati bolognesi, Antonio Bertin e Antonio Vitale, hanno convinto i vertici dell'Istituto nazionale di fisica nucleare a ospitare nel laboratorio del Gran Sasso un esperimento di fusione «fredda» sul modello tracciato da Jones, insieme con un altro che ha origine dalla ricerca di tre scienziati milanesi (Fiorini, Bianchi e Bellotti). In quest'antro isolato dal mondo, avvolto nel «silenzio cosmico» (quasi totale assenza di radioattività naturale), i fisici italiani tenteranno di rubare il segreto del fuoco che accende il Sole.

Fabrizio Lentini

danza a rischio dell'ospedale provinciale «Umberto Primo» di Siracusa sono stati rinviati a giudizio dal giudice istruttore Roberto Campisi per omicidio colposo. Secondo l'accusa il dottor Gaetano Italia, 60 anni, primario del reparto, e il dottor Evangelista Rizza 45 anni, non avrebbero adottato tutte le misure opportune per evitare complicazioni che si erano manifestate nella giovane puerpera, Maria Runza, morta il 30 giugno del 1986 dopo aver partorito un bambino morto.

L'AGGUATO DI GELA

Tarabba è morto in ospedale

GELA *(roc)* - È morto all'ospedale «Garibaldi» di Catania l'imprenditore Alfonso Tarabba, 46 anni, rimasto gravemente ferito nell'agguato tesogli venerdì scorso a Gela mentre di primo mattino si stava recando a piedi a prelevare la sua macchina in garage, poco distante dalla sua abitazione. Tarabba, contitolare della «Asfalti e bitumi», incensurato, era stato arrestato l'estate scorsa per forti complicazioni nel delitto Emmanuello. Venne scagionato, quindi scarcerato. Assieme a lui era stato arrestato il cognato, Orazio Di Dio, ucciso il 14 marzo scorso. Con Tarabba, che era stato colpito alla testa da almeno tre proiettili, sale a 42 il numero dei morti ammazzati a Gela negli ultimi 18 mesi.

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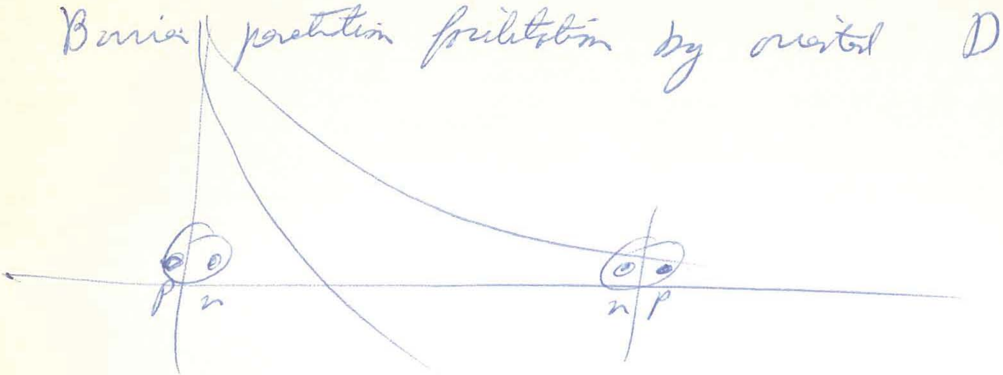
Barrier penetration facilitation by oriented D.



assume that barrier is there $^{(1)}$ because the neutron sticks out exponentially with deuteron wave function, and $^{(2)}$ because center of charge is displaced.

(1) substitutes exponential decay of n ψ_n^2 in d with distance for exponential decay of d ψ_d^2 in barrier. The deuteron is bound with 2.2 MeV and the reduced mass is 0.5 for the n, so ~~as~~ $i\lambda_n = \hbar/\sqrt{2.2 \text{ MeV-AMU}}$

$$\boxed{\frac{p^2}{2m} = E \qquad \frac{\hbar^2 k^2}{2m} = E \qquad \lambda \equiv \frac{1}{k} = \hbar/\sqrt{2mE}}$$

$= \hbar \times 1.5$

at least for this mode, $i\lambda_d = \hbar/\sqrt{2 \cdot 0.5 \text{ MeV amu}}$
$= \hbar \times \cancel{0.8}\ 1$

so one loses by working with the shift.

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(21)

(2) As for orientation, one must project the deuteron onto the state which is $6\% \times 6\% = 3\times10^{-3}$? to obtain a length of 2?? Fermis in the barrier? but $\bar{\lambda}_d = 10^{-27} \times \sqrt{2\times \cancel{10}\, 1.6\times10^{-24} \times \frac{1.6\times10^{-6}}{2}}$

$$= 10^{-27} \times \sqrt{1.6\times10^{-30}}$$

$$= \frac{10^{-27}}{1.3\times10^{-15}} = \underline{\underline{0.8\times10^{-12}\ \text{cm}}} = 8 \text{ Fermis}.$$

So it is good for only 25% or 300× less

ROBERTO MONTI

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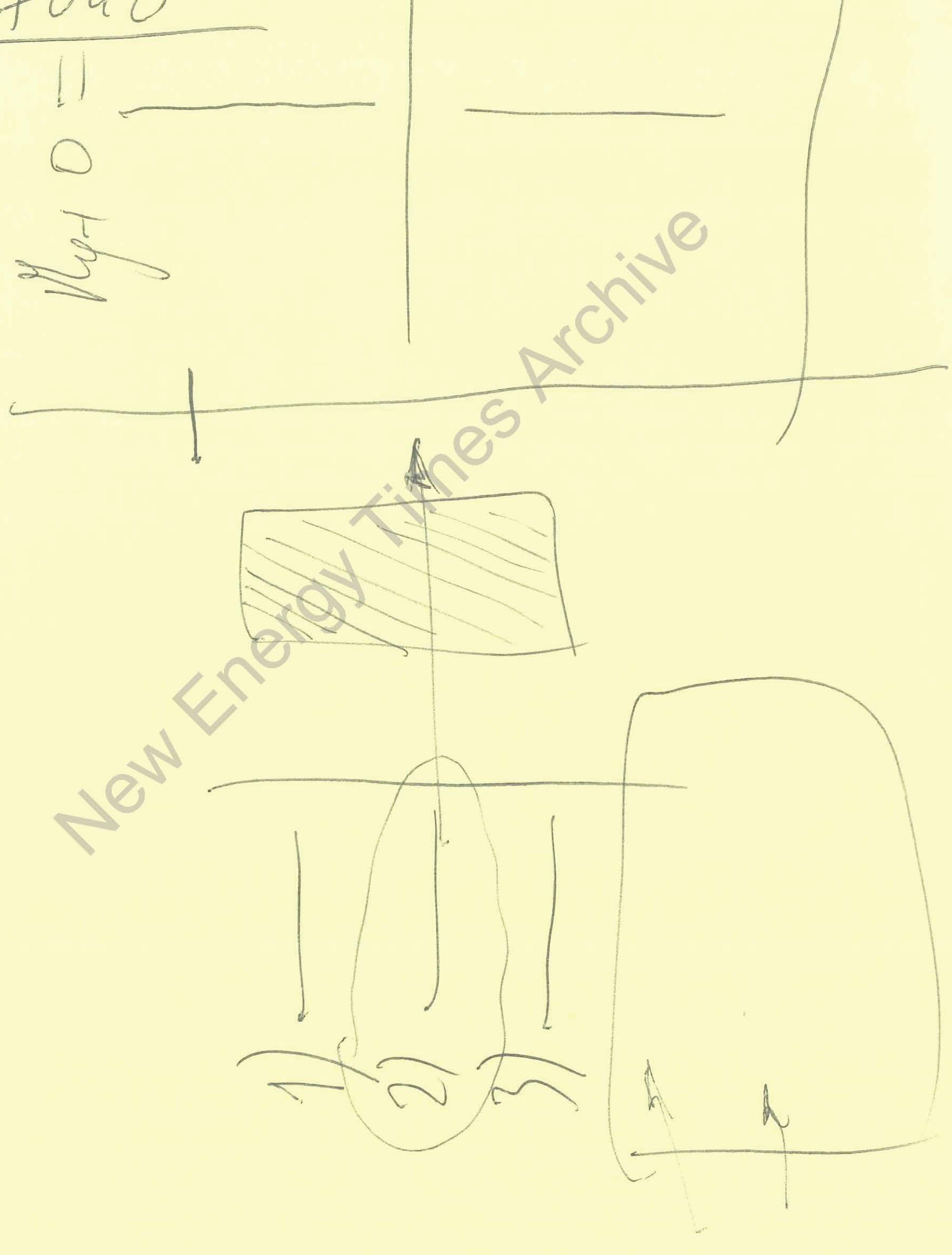

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Hungarian group:

Dr Csikai at (36-52) 15-222

"Neutrons last for about an hour, maybe 2. They can be brought back by cleaning the Pd plate (when it gets ~~clouded~~ clouded, the neutrons go away)": Use high pressure counter, MCA. Electrolyte;

Texas A&M / Charles Martin, Kenneth Marsh, Bruce Gammon.

(Prof John Bockris, a resident expert, with whom Fleischmann had worked long ago).
(409) 845-4947.

At about 15:00 Texas time on Thursday, took their apparatus to the Cyclotron center (where there are very good neutron counters) and switched it on. "Delighted with neutrons -- rose for 5-10 minutes and then fell away for 20 minutes".

As for heat at A&M -- reported by one colleague as 30 w/cm^3. In fact, six Pd rods do not produce heat at all; one reportedly does. Colleague says not "70% excess" but 5%, and ~~two~~ other workers report 1.5% ~~[illegible] [illegible]~~ margin of error. No neutrons, no T, no gamma rays.

04/12/89
②

Harwell (David Williams via J.B.)

Cell-by-cell 1-12, current densities — no heat, no neutrons.

Georgia Tech / (via Rafelski) (602) 621-4212.
James Mahaffey (404) 894-3456

"they baked the cathode and immediately on starting cell saw 600 n/hr)."

Press release 40 c/h bkg → 600 after current was applied.
"within 90 minutes".
baked at [illegible].

? Time, temperature of bake? Size cathode?

? Counts, bkg counts?

? What electrolyte, current density?

? What "oxidize"?

U.U. (via J.B.)

Paper of 03/20 says neutrons detected: neutrons (pp. 3,4), 3x bkg.

But on Sunday 04/09/89 Pons quoted as saying "We've found the neutrons thank God!"

Birmingham / Derek Bogron "Boyron". ? "5 times bkg"
(44-21) 414-4694 office
458-4894 home.

"Can FAX to us"; "has copy of Hungarian data"

04/11/89 ①
17:41

Re Rev Lewis of 03/28/89, page 2, 8,

We need now to evaluate $\frac{\varphi''}{\varphi'^2}$ to see whether it is small.

φ' is monotone.

$$\varphi'' = \frac{d\varphi'}{dr} = 9\times10^5 \frac{d}{dr}\left(\frac{1}{r}-\frac{1}{r_0}\right)^{1/2} = 9\times10^5\left(\frac{1}{2}\right)\left(\frac{1}{r}-\frac{1}{r_0}\right)^{-1/2}\left(-\frac{1}{r^2}\right)$$

$$\boxed{\frac{\varphi''}{(\varphi')^2}} = \frac{9\times10^5}{(9\times10^5)^2}\times\frac{1}{2}\times\left(\frac{1}{r}-\frac{1}{r_0}\right)^{-3/2}\times\left(-\frac{1}{r^2}\right)$$

$$= \frac{1}{1.8\times10^6}\left(\frac{r_0-r}{rr_0}\right)^{-3/2}\times\frac{1}{r^2}$$

$$= 5\times10^{-7}\; r^{3/2}\left(\frac{r_0}{r_0-r}\right)^{3/2}\times\frac{1}{r^2}$$

$$= 5\times10^{-7}\left(\frac{r_0}{r_0-r}\right)^{3/2} r^{-1/2}$$

$$\equiv -\frac{d}{dr}\left(\frac{1}{\varphi'}\right) = -\frac{d}{dr}\left(1.1\times10^{-6}\right)\left(\frac{1}{r}-\frac{1}{r_0}\right)^{-1/2}$$

$$= 1.1\times10^{-6}\left(\frac{r_0-r}{rr_0}\right)^{-3/2}\times -\frac{1}{2}\times -\frac{1}{r^2}$$

$$= \frac{1.1\times10^{-6}}{2}\left(\frac{r_0-r}{r_0}\right)^{-3/2}\frac{r^{3/2}}{r^2} = 5.5\times10^{-7}\left(\frac{r_0}{r_0-r}\right)^{3/2} r^{-1/2} \quad \underline{\underline{\text{yes}}}$$

So for $r\to0$ $\frac{\varphi''}{(\varphi')^2}=1$ at $r=\underline{\underline{30\times10^{-14}}}$,

but for $r\to r_0$ $\frac{\varphi''}{(\varphi')^2}=1$ at $\frac{r_0-r}{r_0}=\left(5.5\times10^{-7}\right)^{2/3}$

or $1-\frac{r}{r_0}= 0.8\times10^{-4}$

04/11/89 ②

17:57

We are then interested in the error in φ,

-- $\varphi' \times \Delta x$ -- pretty small at small x

very " at $r = r_0$

(1)

Steven E. Jones, BYU

Cold nuclear fusion; DOE Adv Energy Projects Div

- scheduled to talk about muon catalyzed fusion; theory work since 1985; expts since 1986.
- "you know the story"

Also $(p+d) \rightarrow He^3 + 5.45\ MeV\ \gamma$

Cold nuclear fusion Sci Am July 1987

0.74 Å D_2 $\lambda_f = 10^{-74} s^{-1}$

$D_2\mu$ $\lambda_f \sim 10^6 s^{-1}$

~~0.25Å~~ $\lambda_f \sim 10^{-23} s^{-1}$

0.25 (3 times more).

$(Dt\mu)$ 180 cycles avg (resonant molecule formation).

S.S. Gershtein and L.I. Ponomarev Phys Lett 72B, 80 (1977)

$\alpha\mu$ sticking meas. W_s^0 initial sticking $(1+0.1 \pm 0.3)10^{-2}$

W_s effective sticking (0.42%)

(stripped by high ρ liquid)

still factor 10 or 20 less than needed for energy,

+ Ponomarev + muon stripping,

17.6 MeV/fusion

at $\frac{20 \times 17.6 \quad 350\ MeV}{130\ MeV\ \mu}$ ~ 2.7:1

$p+d \rightarrow m_r = \frac{2}{3} = \cancel{0.7}\ 0.67$
$d+d \rightarrow 1.0$

(2)

Piezonuclear fusion J Phys G. Nucl Phys March 1988

liquid scintillator / moderator ~ pulse on shape discrim.
also $^6Li + n \rightarrow t + \alpha$ [also seen on PMT]

~~σ~~ $\sigma = \sqrt{f.g. + \alpha^2\, b.g.}$

Gaussian peak amplitude 23 ± 4
half amplitude ~80 – 120

~~would~~ saw 4×10^{-3}/sec at 1% 0.4 γ/sec source
would not have seen 5.45 MeV γ at 0.4 γ/sec created,

Run 6 is best, but system is Σ of all.

"In years of work have not seen heat"

Central pressure 3.6 Mbar
[k smaller by factor $\left(\frac{3}{2}\right)^{1/2} = 1.224$]

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869226 ?

TELEFAX MESSAGE

FROM: Roland Pease
NATURE EDITORIAL, LONDON OFFICE

TO: Dr. R L Garwin
ETTORE MAJORANA INST

SUBJECT: "NEWS y VIEWS"

PAGE ONE OF 3 PAGE(S)

DATE: 11th April

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11th April 1989

Dr R L Garwin
Ettore Majorana Institute
Erice

Thank you for agreeing to write a report on the Erice meeting for News and Views. I presume that the meeting will concentrate on potential experimental artefacts and plausible mechanisms of cold fusion, both of which would be suitable for discussion. I suppose there might also be further experimental 'confirmation' of the results, but unless these tidy up the physics, I suspect they will be less suitable for discussion. It would probably be best if we discuss the proposed content of your article on your your return to New York.

A copy of the News and Views guidelines follows. These indicate the kind of level at which to aim for non-specialists (some review of the basic physics of fusion will needed). They also highlight stylistic points we ask authors to observe. You should aim for about 900 words. I understand that you hope to return you manuscript on Monday 17th April, in which case we will include it in the same issue as the Jones paper (27th April issue, on present schedules) so that you can refer to this as a Nature paper. I will let you know on Friday what the status of the papers is.

I look forward to hearing from you.

Yours sincerely,

Roland Pease (assistant editor).

e6

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JTML to RLG:

I thought you might want to have a hard copy of Dr. Bigeleisen's message left on PHONEMAIL.

1540 RSA1	Marylee Grogan (202) 546-1700	PHONEMAIL. Of International Foundation. She is calling back to see if the ALPS meeting can be changed from 05/05/89 to 05/04/89. **JTML will call and say it is all right with RLG.**
1630 RSA1	Linda Carminucci (914) 694-3725	PHONEMAIL. She has checked to see if there is any low fare by attaching the outbound portion with your flight on the 29th. There is no cheap fare. She is putting it on a round trip NY-ITHACA NY. Call her if there is a problem.
1635 RSA1	Judy Jones (217) 244-0218	PHONEMAIL. She needs a short biography for the workshop at Cornell later this week. It need be only a couple of sentences long. Would we call her and read it over the phone? **JTML will call for her FAX number and send 3-sentence BIOG that was FAXed to S. Leventhal 03/17/89.**
1720 RSA1	Jacob Bigeleisen (516) 632-7905	PHONEMAIL. Of Stonybrook. He has read the preprint of the paper by Fleischmann and Pons. He has been doing some calculations and research on the question of the tritium production and the "fusion" reaction of theirs. They report in the text a tritium content of the heavy water after running their fusion reactor for some time of 100 disintegrations per milliliter of heavy water. Heavy water contains tritium from consecrate?? production or from bomb production, and the amount of tritium in the heavy water depends on the source of

GENERAL GUIDELINES FOR NEWS AND VIEWS AUTHORS

Nature's News and Views section provides a forum in which scientific news can be communicated to a wide audience. News and Views articles should therefore make clear the advance, communicate a sense of excitement and yet provide a critical evaluation of the work in the context of the rest of the field. It is often helpful to ask a colleague from a different discipline to comment on a draft article before submission to *Nature*.

Although diversity of presentation is important, there are some general principles that apply to all News and Views articles.

1. The 'news' should be mentioned in the first paragraph to attract the attention of those who are not experts in the field. The first paragraph should explicitly refer to the paper under discussion and touch on the significance of the work.
2. More detail, background and explanation should follow, including your own 'views'. Articles should not read like textbooks: most readers will have a general scientific background, so although specialized terminology should be avoided, concepts need not be introduced in a simplified manner. Authors should not refer to their own work, except in passing.
3. Figures and diagrams should be used freely to explain the points made. Figures in a 'box' are often a useful way to separate necessary background from the main thread of the argument.
4. Suggestions for titles are welcome but may be changed. Titles should have 25–30 characters (including spaces) and contain no punctuation marks or abbreviations. News and Views articles also have a bold heading of one or two words to define the subject matter of the article and which is used for indexing purposes.
5. References should be kept to a minimum and should ideally be fewer than 10 in number. They should be given superscript numbers and cited sequentially in the text. If the News and Views article is intended to accompany a report in the same issue of *Nature*, the paper should be given a formal reference and be referred to in the text in the form: ...on page xxx of this issue.... References should be listed at the end of the article, and the first and last page numbers of cited papers should be included.
6. Acknowledgements are not allowed, nor are grant and other numbers.
7. Manuscripts should be typed double-spaced and four copies sent. They can be sent to London or Washington and should be addressed to the News and Views Editor, irrespective of who commissioned the article in the case of invited contributions. For other details of manuscript preparation, see *Nature*'s guide to authors, published approximately every three weeks.
8. Addresses: *Nature*, 4 Little Essex St, London WC2R 3LF. Telephone 1-836-6633; facsimile transmitter number 1-836-9934. *Nature*, 1137 National Press Building, Washington DC 20045. Telephone 202-737-2355. Articles and proofs can be sent by facsimile transmitter. Please include a full postal address, a telephone number and, if available, a facsimile transmitter number when submitting manuscripts.
9. A modest fee is paid for all News and Views articles that are published.

MEETING REPORTS

Reports should be delivered as soon as possible after the end of the conference, and within three weeks. They should not be longer than 900 words without prior agreement with the editorial office and are best organized in the following manner.

The first paragraph or two should be clearly aimed at the non-specialist (or specialist from neighbouring disciplines) and should make it plain why the conference is worth reporting. This may be done by reporting the most exciting news heard at the conference or by explaining the overall significance of the conference and the conclusions reached at it. The first paragraph should not be used for a historical introduction to the field; if one is necessary at all it should come in a second paragraph after the 'news' has been presented.

When the conference is first mentioned in the introductory paragraph an asterisk should direct the reader to a footnote where the place and date of the conference are given. The text should not contain any detailed description of the conference (for example, number of people attending, name of chairpersons, number and titles of sessions) or personal impressions of the conference (for example, success of the organization, lavish scale of the banquets, exhaustion resulting from attending numerous parallel sessions). Readers will not want to hear about the conference, they will want to know what the conference was about!

The main body of text can be aimed at a more specialist audience and should contain details of individual talks or groups of talks. Where a particular speaker's contribution is described, his or her name and affiliation (university or institute, not just place name) should be given. These should be placed in brackets so that the report is a flow of ideas rather than names. For example, we prefer that you do not write: "Dr Jack Smith of the Rockefeller University presented data showing that the endonuclease is important..." but write: "The endonuclease is important ...(J. Smith, Rockefeller University)..." You should aim for an article in which all the names in brackets could be removed and still leave an article with a clear theme running through it.

Only the really new and important results should be reported and no attempt should be made to cover every speaker. Where a contribution is reported, though, sufficient detail should be given for it to be properly understood. It is particularly important to avoid writing a large number of very short reports that give little more than the talk titles and do not explain what was really said. It is usually unnecessary to provide a special concluding paragraph as important new conclusions should be reported right away at the beginning of the article. ☐

Spoke with ABM.
30 W/cm³
uncertain
to chem/[illegible]
Bockris
John
(409) 845-4947

Questions → 1. ~~see guys who have seen neutrons,~~ (6 groups)
~~all "ifs & buts"~~

— W~~hat mass of~~ Pd? or Ti?

— ~~Counts, bkg counts.~~

— ~~How long did it persist?~~

Pons → Martin [illegible] can't get a density of it.
70% [illegible]; Texas only 1/7 [illegible]

STEVE,

AS OF THE EVENING 4/10 THE GEORGIA TECH STORY LOOKS VERY BELIEVABLE: THEY BAKED THE CATHODE (PD OR TI?) AND SAW IMMEDIATELY A SIGNAL OF (FUSION?) NEUTRONS AT THE LEVEL OF 600/HOUR.

I WILL FAX A PROGRESS REPORT EVERY 24 HOURS.

[illegible]
→ James Mahaffey
(404)-894-3486

JAN Rafelski
(602) 621-4212

~~1.5 hrs ago -- went to Cyclotron Lab~~
~~& switched on -- [illegible] with neutrons -~~
~~rose for ~ 10 mins & fell for 20 mins~~

~~Hungarian [illegible] Csik for about an hour, maybe 2,~~
~~Csik [illegible]~~ ~~we see - plate got clouded~~
~~age Csicker~~ ~~[illegible] or many -~~
~~Csiko -~~

~~David Williams Harwell~~
~~Martin Fleischmann in~~
~~Brothers [illegible]~~

M. Gerischer

#4
~~Decade~~
~~1926 - 1928~~
~~Patents in Palladium~~
~~catalyzed fusion.~~

~~[illegible]~~

~~all by cell~~
~~1-12, Cement [illegible]~~
~~no heat, no neutrons~~

~~Csiki (36-5245-222~~
~~MCA at high pressure~~

- BYU

Speaking to Stanley & Martin quite a bit. [Pons wealthy, very influential, [illegible] to [illegible] [illegible])
Stanley said 26 W/cm³ & 5X10⁴ n/sec
today, steady [illegible] so [illegible] [illegible] [illegible] [illegible] [illegible]
[illegible] - it [illegible]

Sunday, Charles [illegible]) [illegible] & [illegible] [illegible]
heat (carbon [illegible]); [illegible] [illegible]-to-[illegible] [illegible]
[illegible] piece of Pd -- extra heat and time -- seven [illegible]
[illegible] [illegible] [Don't [illegible] [illegible] [illegible] -- [illegible]
only [illegible] [illegible] on [illegible]-1

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TELEFAX MESSAGE

FROM: Roland Pease
NATURE EDITORIAL, LONDON OFFICE

TO: Dr R L Garwin
ETTORE MAJORANA INST

SUBJECT: "NEWS & VIEWS", EXTRA EDITORIAL COVERAGE OF COLD FUSION in 13 Apr. Issue.

PAGE ONE OF 4 PAGE(S)

DATE: 11th April

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NATURE VOL 338 13 APRIL 1989

Gorbachev needs an answer

Mr Mikhail Gorbachev's visit to London last week has sharpened still further the issue of NATO's tactical nuclear weapons. NATO cannot pretend that the issue is technical and exclusively its own concern.

LAST year's tragic earthquake in Armenia has cast a long shadow. That is the simplest explanation why Mr Mikhail Gorbachev's visit to London last week was probably as much a disappointment for him as for many of those who heard what he had to say. His original plan had been to follow his speech at the United Nations in December, when he made public his plan unilaterally to cull 500,000 people from the Soviet armed services, by travelling to Cuba and then to London. Then, in the interregnum between the Reagan and Bush administrations in Washington, it would have been easier for him to open a fruitful dialogue on arms control. By last week, the momentum had been dissipated. Worse, the British government, now preoccupied with the sharpening dispute within the North Atlantic Treaty Organization (NATO) about the continuing role of nuclear weapons in Europe, was in no mood to respond imaginatively.

The issue is simple, but none the less corrosive on that account. The treaty on missiles of intermediate range (INF) signed at the end of 1987 regulates the removal from Europe of missiles capable of carrying nuclear warheads a distance of 1,500 km or more, but only implicitly regulates missiles of shorter range. (A precondition of INF was the readiness of NATO and the Warsaw Pact to remove tactical nuclear weapons from the scene.) Now, NATO wishes to improve ("modernize") the performance of its tactical nuclear weapons over the next few years. Specifically, there is a plan for replacing the Lance missiles now sited in West Germany with improved rockets capable of travelling 500 km. The Soviet Union disapproves — Gorbachev said so plainly last Friday. So, for different reasons, does West Germany.

Gorbachev's view is that improving the performance of NATO's tactical weapons will break the spirit if not the letter of INF. He said last week that if NATO goes ahead with its planned modernization, the talks on the regulation of conventional arms now under way in Vienna will be jeopardized. NATO's counter-argument is that nuclear weapons remain a necessary part of the defence of Western Europe. At least so long as the intended agreement on conventional forces in Europe remains in the future, nuclear weapons may be the only way of halting an armed incursion. That view is not new: NATO has held to the same doctrine for a quarter of a century. The novelty is twofold — there is now a chance of winning an agreement on conventional forces, while West German voters and thus, perforce, the West German government are increasingly attracted by the even brighter prospect of a more general disappearance of political and military tension in Europe.

Formally, so far as NATO is concerned, the issue is intended to be settled in the summer of this year, after the Bush administration's review of its security interests (and the associated defence budget) is complete. But that is more than NATO's managers can reasonably expect. If anything, the reluctance of West German voters to support governments which agree to give house-room to modernized weapons is more likely to grow than to melt away. A further difficulty is that the Vienna negotiations will not make sense if aircraft and other remote means of delivering bombs (nuclear or otherwise) are excluded, which is what NATO asks.

That is why the need now is for a constructive compromise. Gorbachev's protest that NATO's insistence on improved nuclear weapons in Europe would jeopardize the Vienna negotiations was quickly countered, in London and Washington, by a simple reiteration of the need to modernize outdated nuclear weapons. Why not, instead, acknowledge that the need for battlefield nuclear weapons in Europe would be diminished if the Vienna talks succeed, and thus make the planned changes contingent on failure at Vienna? That way, Gorbachev and his counterparts in the West could be saying much the same thing, while the important negotiation at Vienna would be invested with a sense of urgency. □

Disorderly publication

There is no reason why discoveries should not first be published in daily newspapers, but there are drawbacks.

THE great fuss about cold fusion over the past few weeks raises important questions about the scientific literature and its function to which the scientific community should give urgent thought. Reports that nuclear fusion had been brought about in an electrochemical cell first appeared in two financial newspapers — the *Wall Street Journal* and the *Financial Times* — on 23 March. Professors Martin Fleischmann (Southampton) and Stanley Pons (Utah) were reported to have accomplished fusion in an electrolytic cell.

Soon it also became known that a group at Brigham

Young University led by Professor Steven E. Jones was ready with comparable but not identical observations. The University of Utah promptly held a press conference at which, among other things, an article by Fleischmann and Pons was said to have been submitted to *Nature*. In the event, the first article to arrive was from the Brigham Young group, but one from Fleischmann and Pons reached us later; an extended version of it was published last week under the heading "preliminary note" in the *Journal of Electroanalytical Chemistry* (see page 537).

It is no disrespect to any of those concerned to compare the dilemma created for *Nature* by these events to that occasioned a year ago by the article in which Professor Jacques Benveniste and colleagues claimed that indefinitely diluted reagents retain their biological effectiveness. The claim flies in the face of orthodox belief, but the data available are insufficient for a careful judgement of its validity. But on this occasion, *Nature* has followed standard procedures. Both articles have been sent to referees, each is now being revised in the light of the many comments that have been made. Many of these would normally require extensive reinvestigation. In the normal course of events, weeks or months might go by. Researchers are used to delays of that kind, especially when they have important things to say. The process is beneficial because it improves the quality and reliability of what is published.

What is to happen now? Public and professional curiosity now require rapid publication, but there is only a small chance that either group of authors can make all the amendments asked of them in a short time. So should publication be postponed until they can do so? There is a sense in which that would not matter: the information is already in circulation. But publication is more than merely the circulation of information to those with access to the appropriate networks. General availability is crucial. That is why revised versions of one or other or of both articles will be published later in the month. In these exceptional circumstances, they will be accompanied by such comments of the referees as remain pertinent.

None of this implies that the peer-review system is infallible. Those who live with it know that it is shot through with imperfections. There is, for example, a danger that it induces too much uniformity in the literature. But the system is a powerful means by which good ideas are made better on their way into print. The practical question for the scientific community, and for journals in particular, is to adapt the system (and the process of publication) more swiftly to the steady improvement of communications.

Nobody can sensibly complain about those developments. But there remains an immense difference between the discussions thus stimulated within the scientific community and the broadcasting of claims not fully tested. Fleischmann and Pons said at their press conference that their work was in danger of "leaking out", much as Benveniste last year cited reports in *Le Monde* as a cause for urgency. But the authors of an experiment are best placed to determine when and how news of their work is published generally. It is naturally difficult to bottle up exciting news, but impatience is a poor guide to action. The greater the importance of a discovery seems, the longer it should be worthwhile waiting to see it properly established. □

Pan-Europe university?

The Swiss are nursing the idea of creating a European university and should be encouraged.

PLANS to turn the European Economic Community (EEC) into a genuine common market at the end of 1992 may not yet have done much to change economic behaviour in the 12 countries directly involved, but they hav: had a powerful influence on outsiders. Japanese and US companies are busy building factories in member states or forming business partnerships with companies registered there. Governments are similarly impelled by a sense of exclusion to seek membership of the EEC: Turkey has formally applied, Austria says it will and Norway (for the second time) is brooding. The way things are going, by 1992 there may be only one European outsider left outside — Switzerland.

This prospect keeps stoical Swiss awake at nights. Their difficulty is plain: the constitution of the Swiss Federation, with its devolution of power from the centre, could not be changed without changing Switzerland. Yet it was already plain last year (see *Nature* 336, 323 *et seq.*; 1988) that anxiety about the emerging single market had become an influence in Swiss planning of research as well as a source of fear that Switzerland may become an amalgam of holiday resort and retirement home for nationals who choose to spend their working lives elsewhere. But anxiety is a catalyst of the imagination, whence the notion now gaining ground that Switzerland, within the confines of its constitution, might make a distinctive contribution to the development of Europe by creating a university on a grand European scale.

This is a daring notion, but none the worse for that. Europe, taken as a whole, has a commendable diversity of universities, but most of them are bound to seem parochial by the yardsticks of the pan-Europeans. But Switzerland has a cosmopolitan tradition rivalled only by that of the Netherlands and might — so the calculation goes — be able to build from its existing institutions a distinctively European university that can be counted among the half dozen or so outstanding universities in the world. For the time being, of course, this is just a dream, but one which many influential Swiss are eager to explore. Altruism is not the only driving-force, of course: to be the site of an acknowledged leader among European universities would be an assurance against isolation, while a strong research programme would undoubtedly engender spin-off. But it is a notion that universities elsewhere in Europe should be ready to welcome. □

Hot-footed towards cold fusion

The only published account so far of thermonuclear fusion in an electrochemical cell raises, as its authors say, "more questions than it provides answers".

THERMONUCLEAR fusion, which allows stars to shine, must also occur in more humdrum circumstances. Put two deuterons together in a deuterium molecule, for example, and there is a small chance that they will fuse together spontaneously, either to produce a tritium nucleus and a proton or a helium-3 nucleus and a neutron. Perhaps luckily, the chance is astronomically small, maybe 10^{-70} per molecule per second. At that rate, there would be roughly one fusion event an hour in a quantity of molecular deuterium roughly as massive as the Galaxy.

The reason why muons have been advocated as catalysts of fusion is that their mass (some 200 times greater than that of an electron) makes for a smaller D_2^- ion, thereby increasing the wave function of one deuteron at the position of the other and so increasing the still-random chance of fusion.

The great excitement in the past three weeks about the prospect that thermonuclear fusion has been accomplished electrochemically is an extension of this principle. The article by M. Fleischmann and S. Pons which appeared last week (*J. Electroanalyt. Chem.* **261**, 301; 1989) begins from the view that conditions favourable for fusion may be created electrochemically by exploiting the familiar affinity of palladium for hydrogen (in all its isotopic forms).

As described, the experiments seem straightforward. Use a platinium anode, a palladium cathode and an electrolyte which is a heavy-water solution of deuterated lithium hydroxide. To prevent the deuterons bubbling away as deuterium, arrange that there is a substantial negative over-voltage on the cathode. The effect is that deuterons accumulate in the palladium lattice, and will continue to do so until their conversion into molecules resumes.

That point, Fleischman and Pons argue, is determined by the chemical potential of the deuterons in the palladium lattice, which is itself determined by the negative over-potential on the cathode. They use the term "galvanostatic compression" to describe this process of forcing deuterons into the palladium.

Three kinds of measurements are described, one of which is simple calorimetry. The question is whether the heat produced in such a cell is greater than expected, which is most simply that calculated by multiplying together the current and the voltage.

The article describes measurements with cells containing electrodes of different shapes and sizes. For three-rod electrodes of different diameters, the specific rate of excess heat production is reported to have increased with increasing current density and with the thickness of the rod (4 mm at its maximum), both of which argue for a phenomenon liberating excess heat in which the bulk of the palladium is involved. There is also an account of how, in one experiment, the cathode (a 1 cm cube of palladium) vaporized.

True fusion should of course be recognizable in other ways, by the detection of its by-product particles for example. The authors report measurements of γ-rays presumed to have occurred by the reaction of neutrons from fusion reactions with protons in the water-bath surrounding the electrolytic cell and also the detection of neutrons (at roughly three times the intensity of the cosmic-ray background) while one experiment was running. They have also looked for (and claim to have found) tritium in the residual electrolyte in a cell.

So does this not add up to a proof of fusion? From the details published so far, no-one can say. The nuclear evidences offered are all close to the edge of what is measurable. The measurement of tritium production, for example, looks convincing, but there are pitfalls. Any quantity of deuterium-rich water invariably contains the heavier isotope in some quantity. One is looking for a small increase in tritium content, rather than its mere presence where there was none before. Similarly, there are other sources of neutrons and gamma rays besides fusion reactions (notably cosmic rays and natural radioactivity), and these must be convincingly subtracted.

Even taking the γ-ray and neutron production at face value, far too few of either are recorded to explain the claimed heat generation by known fusion reactions. In one of the experiments described, the rate of production of excess heat suggests that there should be between 10^{11} and 10^{14} fusion events a second, but the nuclear physics data suggest that known fusion reactions (leading either to tritium or ^{3}He) account for only about 10^4 of these.

This is the most serious impediment to belief. The heat production in these electrolytic cells is more than a million times greater than nuclear by-products would suggest it can be. This leads Fleischmann and Pons to say that "the bulk of the energy release is due to a hitherto unknown nuclear process or processes". What, one wonders, can that process be? Is it likely to have escaped the attention of nuclear physicists in the past half century?

Sceptics, in the circumstances, will be quick to ask whether the necessary subtle subtractions from the gross energy output have been accurately made — the electrical energy put in, the heat that would have been produced in a normal electrolytic cell (with ordinary hydrogen instead of deuterium) and so on. Because Fleischmann and Pons spend days, perhaps weeks, loading their electrodes with deuterium, there is plenty of energy stored up in their system before any return is obtained. Even the much-described "explosion" of one of their electrodes may be explicable by chemistry.

By now, the tiny band of *Nature* referees which has scrutinized the two articles submitted for publication has been joined by many other people. Further information about the issues which have given them difficulty will be given in a further issue of *Nature*. Numerous attempts to replicate the measurements have produced only one positive claim (see page 529). One common complaint has been that the data provided are insufficient to allow faithful reproduction of the measurements reported. The Fleischman and Pons article as now published is a starting-point for other experiments, but says less than the average chemist needs to know.

A more perplexing circumstance is that the authors are reported to have said that some cells work and others do not, although that may not be surprising when so little is known of the system as a whole. The idea that cells produce excess heat only after they have been running for long periods makes sense if a palladium lattice must be charged with deuterons before fusion occurs at anything like a decent rate, but one is left wondering whether the amount of charging needed can be predicted in advance for a given cell geometry and voltage, as one would expect if such a straightforward explanation were the only one. What seems plain is that arguments like these will continue for a long time. □

Date: Tuesday, 11 April 1989 1447-EST
From: STEINH@PENNDRLS.UPENN.EDU
Subject:
To: RLG2@IBM.COM

I look forward to hearing the news from Erice! Please send me an account when you get back.

Dick Murtagh

----- FUSION FORUM appended at 12:20:41 on 89/04/10 GMT (by MITCH at LEXCJN1) -
Subject: NPR Report

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Gary A. Mitchell

----- FUSION FORUM appended at 12:39:13 on 89/04/10 GMT (by RVFIRTH at CLTVM3)
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Ref: Append at 16:26:11 on 89/04/06 GMT (by ZIEGLER at YKTVMV)

This thought has no doubt already occurred to you folks in Yorktown, but on the off chance that it hasn't: Suppose there is something to the theory that fusion occurs by tunneling only after all of the interstitial sites are occupied. According to this theory, the incoming deuterons are briefly occupying the same energy states as those already in the lattice since there are no other available locations. If this is true, the form factor of the palladium cathode, especially the surface area/volume ratio and absolute thickness is likely to be critical. If the deuterons are easily able to tunnel out of the lattice, into the solution or into your window, then fusion may be greatly reduced. If the above is occurring, a large "if" admittedly, then thin foil would be the worst possible choice for a cathode. A large sphere would be more fusion efficient, perhaps too efficient judging by the results of the cube experiment (although the "meltdown" was probably a conventional chemical reaction, not excessive fusion).

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Re: Does Li react violently with Pd?

This note contains speculation relating to the role of the lithium (from the LiOD electrolyte) in the Pons and Fleischmann observation that "a portion of the cathode fused (melting point 1554 C), part of it vaporized and the cell and contents...were destroyed" as a result of the heat of the reaction in one instance.

According to Moffet's handbook on binary alloys, the phase diagram of Pd-Li includes Pd-rich phases at Pd(7)-Li, Pd(2)-Li and Pd-Li, as well as several Li-rich phases. The melting points of these phases are ca. 1500 C, ca.950 C, and ca. 650 C, respectively. So melting at substantially lower, but nonetheless impressive temps could have occurred if Pd-Li alloys had formed during the electrolytic charging.

Elliott's supplement to Hansen's Constitution of Binary Alloys does not include Pd-Li, but does contain the following note about Pt-Li: "contamination-free Li and Pt react in a violent exothermic reaction at 540 C in vacuum or an inert atmosphere to form the compound LiPt2, which is FCC ...." The phase diagram in Moffet for Pt-Li has the same M-rich phases as the Pd-Li phase diagram, where M is Pt or Pd (melting points not given for the Pt-Li phase diagram).

Would it be conceivable that a Li-Pd "violently exothermic" reaction occurred in the Pons and Fleishmann experiment, as occurs at higher temp in the Li-Pt system? The electrolysis occurring at the cathode would be a reducing environment, promoting contamination-free surfaces in the sense of being free of oxide surface layers, perhaps thereby promoting the reaction.

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Bob Miller, Yorktown

----- FUSION FORUM appended at 17:00:23 on 89/04/10 GMT (by ROBERT at LOSANGEL)
Subject: Heavy Water

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My wife and I had lunch with a chemist/physicist and his wife. As we we were talking about what 'we do', I learned that my guest, Mr. Robert Cohen, is President of Enterprise Chemical Corporation and his group has developed a catalyst to be used in the cheap/efficient manufacture of heavy water. They have acquired American patents and are currently trying to get international patents. An American company - Air Products, and Canadian - Ontario Power, have expressed interest in their catalyst because of the use of heavy water as a moderator in nuclear fission reactors.

I'm sure IBM has no interest in going into the business of manufacturing heavy water, but if any of you guys has an interest in this catalyst, Rob promised to send me a copy of his paper. Anyone?

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R.S. (Bob) Prentice - LOSANGEL/ROBERT

Date: Monday, 10 April 1989 0956-EST
From: STEINH@PENNDRLS.UPENN.EDU
To: RLG2@IBM.COM

Don't get me wrong. I've gone through the naive numerics and I agree that it seems very unlikely that there can be a fusion effect. I strongly agree that any heat effect has to be chemical. I am just saying that there may be collective motions in the lattice that MIGHT enhance fusion (albeit not to the breakeven point, most likely). There might be collective diffusive processes. Also, these systems do undergo structural transformations at various critical densities.

When you saying you are going to meet in Erice on Wednesday, which Erice do you mean? Sicily?

04/16/89
①

"Total number of atoms of D in the domains" = 14.6×10^{23}

atoms of D in D_2O/cm³ $= \frac{1.0}{18}\times\frac{6.023\times10^{23}}{\cancel{18}}\times 2 = 6.67\times10^{22}$

Volume of domain $= \frac{14.6\times10^{23}}{6.67\times10^{22}} = 21.9$ cm³.

They conclude (p8.) that $D+D \rightarrow T+p$ takes place at a rate of $1.2\times10^{4}\,s^{-1}$

But the number of atoms of T in a 22 cm³ domain with (100 − 40) dpm/ml = 60 dpm/ml T decay ~~rate is~~ $\dot{y}$ is N and that.

is $y = \lambda\frac{N}{V} = \frac{1.76\times10^{-9}N}{21.9} = \frac{60}{60}$ dps

$\lambda = \frac{1}{18\,\text{yrs}} = \frac{1}{18\times365.3\times86{,}400} = \frac{1}{5.7\times10^{8}\,\text{sec}} = 1.758\times10^{-9}$

so that $N = 12.4\times10^{9}$ atoms.

at $\dot{T} = 1.2\times10^{4}\,s^{-1}$ accumulation time is $\tau\cdot\dot{T} = N$

so that $\tau = \frac{12.4\times10^{9}}{1.2\times10^{4}} = 10.3\times10^{5}\,s =$ 286 hrs

[revised] R.L. Garwin
From S.E. Jones on board
TW 840 04/10/89

Submitted only to *Nature*

OBSERVATION OF COLD NUCLEAR FUSION IN CONDENSED MATTER

S.E. Jones, E.P. Palmer, J.B. Czirr, D.L. Decker, G.L. Jensen, J.M. Thorne, and S.F. Taylor

Departments of Physics and Chemistry
Brigham Young University
Provo, Utah 84602
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J. Rafelski
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Tucson, Arizona 85721

March 23, 1989

Fusion of isotopic hydrogen nuclei is the principal means of producing energy in the high-temperature interiors of stars. In relatively cold terrestrial conditions, the nuclei are clothed with electrons and approach one another no closer than allowed by the molecular Coulomb barrier. The rate of nuclear fusion in molecular hydrogen is then governed by the quantum-mechanical tunneling through that barrier, or equivalently, the probability of finding the two nuclei at zero separation. In a deuterium molecule, where the equilibrium separation between deuterons (d) is 0.74 A, the d-d fusion rate is exceedingly slow, about 10^{-74} per D_2 molecule per second [1].

By replacing the electron in a hydrogen molecular ion with a more massive charged particle, the fusion rate is greatly increased. In muon-catalyzed fusion, the internuclear separation is reduced by a factor of approximately 200 (the muon to electron mass ratio), and the nuclear fusion rate correspondingly increases by roughly *eighty orders of magnitude.* Muon-catalyzed fusion has been demonstrated to be an effective means of rapidly

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inducing fusion reactions in low-temperature hydrogen isotopic mixtures [2].

A hypothetical quasi-particle a few times as massive as the electron would increase the cold fusion rate to readily measurable levels, about 10^{-20} fusions per d-d molecule per second [1]. Our results imply that an equivalent distortion of the internuclear hydrogen wavefunction can be realized when hydrogen isotopic nuclei are loaded into metals under certain conditions. We have discovered a means of inducing nuclear fusion without the use of either high-temperatures or radioactive muons.

Indirect Evidences

Observations of geological ^{3}He suggested to us original directions for laboratory investigations of nuclear fusion in condensed matter. ^{3}He is produced by the following fusion reactions:

$$\mathrm{p} + \mathrm{d} \longrightarrow {}^3\mathrm{He} + \gamma(5.4\,\mathrm{MeV}). \tag{1}$$

$$\mathrm{d} + \mathrm{d} \xrightarrow[\sim 50\%]{} {}^3\mathrm{He}(0.82\,\mathrm{MeV}) + \mathrm{n}(2.45\,\mathrm{MeV}) \tag{2a}$$

$$\xrightarrow[\sim 50\%]{} \mathrm{t}(1.01\,\mathrm{MeV}) + \mathrm{p}(3.02\,\mathrm{MeV}) \tag{2b}$$

Tritium (t) decays with a 12.4-year half-life to produce ^{3}He. The well-established high ratio of ^{3}He/^{4}He in solids, liquids, and gases associated with volcanoes and other thermal areas [3] suggests fusion as a possible source for the ^{3}He.

To estimate a rate for fusion in the earth, we assume that the known flux of ^{3}He out of the mantle, 2×10^{19} ^{3}He atoms/second [4], arises from p-d fusion occurring uniformly in the mantle water reservoir, taken as $\sim 1.4 \times 10^{24}$ g [R. Poreda, private communication]. Since each p-d fusion produces one ^{3}He atom, and since the isotopic abundance of deuterium in water is about 1.5×10^{-4} deuterons/proton, we infer a geological fusion rate constant of:

$$\lambda_f \sim \frac{2 \times 10^{19}\ {}^3\mathrm{He\ atoms/sec}}{1.4 \times 10^{43}\ \mathrm{deuterons}}$$
$$\sim 10^{-24}\ \mathrm{fusions\ d^{-1}\ s^{-1}} \tag{3}$$

This rate is fifty orders of magnitude larger than that expected in an isolated HD molecule, and fusion at this rate could be detected if reproduced in the laboratory.

Cold nuclear fusion may be important in celestial bodies other than the earth. Jupiter, for example, radiates about twice as much heat as it receives from the sun. It is interesting to consider whether cold nuclear fusion in the core of Jupiter, which is probably metallic hydrogen plus iron silicate, could account for its excess heat. Heat is radiated at an approximate rate of 10^{18} W, which could be produced by p-d fusions occurring at a rate of $10^{30}\mathrm{s}^{-1}$ [1]. Assuming a predominately hydrogen core of radius 4.6×10^9 cm, having a density ~ 10 g/cm^3 and a deuteron/proton ratio of roughly 10^{-4}, we deduce a required p-d fusion rate of $\lambda_f \sim 10^{-19}$ fusions d^{-1} s^{-1} if all the heat derives from fusion. Catalyzed nuclear fusion at this rate could be readily measured in the laboratory.

Further evidence for cold nuclear fusion in condensed matter comes from studies of ^{3}He and ^{4}He in metals. Several scientists have reported high ^{3}He in metal crucibles and foils [H. Craig, R. Poreda, A. Nier, private communications], consistent with in-situ formation by means of cold piezonuclear fusion. In particular, Mamyrin et al. [5] report the occurrence of spotty, high concentrations of ^{3}He in a number of metal foils. Electrolytic refining of the metals could have provided conditions for the cold nuclear fusion reactions (1) and possibly (2). Among several possible explanations for the observations, the authors suggest an "analog" of muon catalysis. [5]

Detection of Cold Fusion Neutrons

The considerations outlined above led to laboratory experiments performed at Brigham Young University to determine whether cold nuclear fusion can actually occur in condensed matter. We now report the observation of deuteron-deuteron fusion at room temperature during low-voltage electrolytic infusion of deuterons into metallic titanium or palladium electrodes. The fusion reaction ($2a$) is evidently catalyzed as d$^+$ and metal ions from the electrolyte are deposited at (and into) the negative electrode. Neutrons having approximately 2.5 MeV energy are clearly detected with a sensitive neutron spectrometer. The experimental layout is portrayed in Figure 1.

The neutron spectrometer, developed at Brigham Young University over the past few years [6], has been crucial to the identification of this cold fusion process. The detector consists of a liquid organic scintillator (BC-505) contained in a glass cylinder 12.5 cm in diameter, in which three lithium-6-doped glass scintillator plates are embedded. Neutrons deposit energy in the liquid scintillator via multiple collisions and the resulting light output yields energy information. Upon reaching low energies, the neutrons are scavenged by lithium-6 nuclei where the reaction $n + {}^6\mathrm{Li} \rightarrow t + {}^4\mathrm{He}$ results in scintillations in the glass. Pulse shapes from the two media differ; the two distinct signals are registered by two photomultiplier tubes (whose signals are summed). A coincidence of signals from the two media within 20 μsec identifies incoming neutrons which stop in the detector.

An energy calibration of the spectrometer was obtained using 2.9 and 5.2 MeV neutrons, generated via deuteron-deuteron interactions at 90° and 0°, respectively, with respect to the deuteron beam from a Van de Graaff accelerator. The observed energy spectra show broad structures which imply that 2.45 MeV neutrons should appear in the multi-channel analyzer spectrum in channels 45-150. Stability of the detector system was checked between data runs by measuring the counting rate for fission neutrons from a broad-spectrum californium-252 source. We have performed extensive tests proving that our neutron counter does not respond preferentially in this pulse height range to other sources of radiation such as thermal neutrons.

Background rates in the neutron counter are approximately 10^{-3} s^{-1} in the energy region where 2.5 MeV neutrons are anticipated. By comparing energy spectra from gamma and neutron sources we have determined that approximately three-fourths of the observed background events arise from ambient neutrons and one-fourth from accidental coincidences of gamma-ray events. The gamma ray background comes mainly from radioactive radium and potassium in the surrounding materials. We attribute the ambient neutrons to cosmic ray sources. Even though the typical neutron-evaporation spectrum (at birth) has a broad maximum near 2.5 MeV [7], monte carlo calculations show that moderation in the source medium (predominately shielding which surrounds the detector) will wash out this structure and produce a featureless background spectrum as observed (see Fig. 2).

During the search for suitable catalytic materials, we developed the following (unoptimized) prescription for the electrolytic cells. The electrolyte is a mixture of ~160 g deuterium oxide (D_2O) plus various metal salts in ~0.1 g amounts each: $FeSO_4 \cdot 7H_2O$, $NiCl_2 \cdot 6H_2O$, $PdCl_2$, $CaCO_3$, $Li_2SO_4 \cdot H_2O$, $NaSO_4 \cdot 10H_2O$, $CaH_4\ (PO_4)_2 \cdot H_2O$, $TiOSO_4 \cdot H_2SO_4 \cdot 8H_2O$, and a very small amount of AuCN. The pH is adjusted to pH $\lesssim$ 3 with HNO_3.

Titanium and palladium, initially selected because of their large capacities for holding hydrogen and forming hydrides, were found to be effective negative electrodes. Individual electrodes consisted of approximately 1 g purified "fused" titanium in pellet form, or 0.5 g of 0.25 mm thick palladium foils, or 5 g of mossy palladium. Typically 4-8 cells were used simultaneously. The palladium pieces were sometimes re-used after cleaning and roughening the surfaces with dilute acid or abrasives. Hydrogen bubbles were observed to form on the Pd foils only after several minutes of electrolysis, suggesting the rapid absorption of deuterons into the foil; oxygen bubbles formed at the anode immediately. Gold foil was used for the positive electrodes. DC power supplies provided 3-25 volts across each cell at currents of 10-500 mA. Correlations between fusion yield and voltage, current density, or surface characteristics of the metallic cathode have not yet been established.

Small jars, approximately 4 cm high × 4 cm diameter, held ~ 20 ml of electrolyte solution each. The electrolytic cells were placed on or alongside the neutron counter, as shown in Figure 1. The cells are simple and doubtless far from optimum at present. Nevertheless, the present combination of our cells with the neutron spectrometer is sufficient to establish the phenomenon of cold nuclear fusion during electrolytic infusion of isotopic hydrogen into metals.

Figure 2 displays the energy spectrum obtained under conditions described above, juxtaposed with the (scaled) background spectrum. We acquired about twice as much background data as foreground data. Assuming conservatively that all deviations from background are statistical fluctuations, we scale the background counts by a factor of 0.46 to match the foreground counts over the entire energy range as shown in figure 2. A feature in channels 45-150 still rises above background by nearly four standard deviations. This implies that our assumption is too conservative and that this structure represents a real physical effect. After re-scaling the background by a factor of 0.44 to match the foreground level in regions below and above this feature, the difference plot (Figure 3) is obtained. It shows a robust signal centered near channel 100 of over five standard-deviation statistical significance. A Gaussian fit to this peak yields a centroid at channel 101 with a sigma of 28 channels and an amplitude of (23.2 ± 4.5) counts. ***This is just where 2.5 MeV fusion neutrons should appear in the spectrum*** according to our calibration. The fact that a significant signal appears above background with the correct energy for d-d fusion neutrons ($\sim$ 2.5 MeV) provides strong evidence that room temperature nuclear fusion is occurring at a low rate in the electrolytic catalysis cells.

Fusion Rate Determination

It is instructive to scrutinize the fourteen individual runs which enter into the combined data discussed above. Figure 4 displays, for each run, the ratio of foreground count rate in the 2.5 MeV-energy region with background rates obtained for each run. Background rates were improved upon during the experiments, so we plot the data in terms of foreground-to-background ratios rather than absolute rates. (In one set of data for which the system was kept as untouched as possible so that background rates were constant, we are able to directly extract an average observed rate for 2.5 MeV neutron production of (6.2 ± 1.5) x 10^{-4} fusions/sec.)

Run 6 is particularly noteworthy having a statistical significance of approximately 5 standard deviations above background. Fused titanium pellets were used as negative electrodes with a total mass of about 3 g. The neutron production rate increased after about one hour of electrolysis. After about eight hours, the rate dropped dramatically as shown in the follow-on run 7. At this time, surfaces of the Ti electrodes showed a dark gray coating. An analysis using electron microscopy with a microprobe showed that the surface coating was mostly iron, deposited with deuterons at the cathode. The same phenomenon of having the neutron signal drop after about eight hours of operation appears in run 13 followed by run 14. Runs 13 and 14 use the same eight electrochemical cells, and again the negative electrodes developed coatings after a few hours of electrolysis. These observations suggest the importance of surface conditions on the cold fusion process. Indeed, wide variations in surface conditions are anticipated in operating electrochemical cells with numerous ionic species, and these variations may account for the fluctuations in the signal level which are evident in Figure 4. In particular, the observed "turning off" of the signal after $\sim$ 8 hours may account for a low signal-to-background ratio in runs 1 and 3, in that a few-hour signal may have been overwhelmed after a long ($\sim$20 hour) running time.

When run 10 started with rates substantially above background, we stopped the run and removed half of the electrochemical cells as a test. The neutron production rate dropped off as expected (run 11). In determining the statistical significance of the data, we included runs 1, 3, 7, 11 and 13, even though we see a systematic reason for their low-foreground-to-background ratios as explained above. Run 8, shown in Figure 4, was inadvertently lost from the magnetic storage device and could not be included in Figures 2 and 3. This does not change our conclusions.

After seeing the fusion neutron signature, we made extensive unsuccessful efforts to generate fake 2.5 MeV neutron "signals" by using various gamma and neutron sources and by turning auxiliary equipment on and off. (Neutron producing machines such as the Van de Graaff accelerators were off during all foreground and background runs.) Numerous background runs were made using operating cells containing standard electrodes and electrolytes except that H_2O replaced the D_2O; other background runs were made using both new and previously used standard cells containing D_2O but with no electrical current. All background runs are featureless, excluding the possibility that cosmic ray-associated muons or neutrons cause the observed signal. In particular, a temporal variation of the cosmic-ray-induced background cannot account for the observed signal. Cosmic-ray muons would be rapidly scavenged by high-Z nuclei so as to reduce muon-catalyzed d-d fusion to a negligible level [2].

The cold nuclear fusion rate during electrolytic fusion is estimated specifically for run 6 (Figure 4) as follows:

$$\text{Fusions per deuteron pair} = \left(\frac{R}{\epsilon}\right) \Big/ \left(M \times \frac{d}{2M}\right) \tag{4}$$

where the observed fusion rate $R = (4.1 \pm 0.8) \times 10^{-3}$ fusions/s; the neutron detection efficiency, including geometrical acceptance, is calculated using a monte carlo neutron-photon transport code [8] to be $\epsilon = (1.0 \pm 0.3)\%$; $M \simeq 4 \times 10^{22}$ titanium atoms for 3 g of titanium; and the deuteron-pair per metal ion ratio $\frac{d}{2M} \approx 1$ is based on the assumption that nearly all tetrahedral sites in the titanium lattice are occupied, forming the $\gamma{-}\mathrm{TiD}_2$ hydride. Then the estimated cold nuclear fusion rate by equation (4) is

$$\lambda_f \sim 10^{-23} \ \text{fusions/deuteron pair/second.} \tag{5}$$

If most fusions take place near the surface, or if the titanium lattice is far from saturated with deuterons, or if conditions favoring fusion occur intermittently, then the inferred fusion rate must be much larger, perhaps 10^{-20} fusions/d-d/second.

We note that such a fusion rate could be achieved by "squeezing" the deuterons to about half their normal (0.74 Å) separation in molecules. That such rates are now observed in condensed matter suggests catalyzed "piezonuclear" fusion as the explanation [1]. A possible cause is that quasi-electrons form in the deuterated metal lattice having an effective mass a few times that of a free electron. Isotopic hydrogen is known to accumulate at imperfections in metal lattices [9], and local high concentration of hydrogen ions might be conducive to piezonuclear fusion. Since we have not seen any evidence for fusion in equilibrated, deuterated metals or compounds such as methylamine-d_2 deuteriochloride or ammonium-d_4 chloride, we conclude that non-equilibrium conditions are essential. Electrolysis is one way to produce conditions which are far from equilibrium.

It seems remarkable that one might influence the effective rate of fusion by varying external parameters such as pressure, heat and electromagnetic fields, but just such effects are confirmed in another form of cold nuclear fusion: muon-catalyzed fusion [10].

Conclusions

The correlation of ideas regarding cold piezonuclear fusion with observations of excess helium-3 in metals and in geothermal areas of the earth led to our experimental studies of fusion in electrochemical cells which began in May 1986. The electrolyte containing various metal salts was developed to a considerable extent based on geochemical considerations (many ionic species are present in the earth's mantle). This rather complex electrolyte was retained following extensive development and testing of our neutron spectrometer. The presence of a fusion neutron signal was then consistently reproduced, although the fusion rate clearly varies (Fig. 4) showing that complex conditions in the cell are not yet fully characterized. Now that our exploratory searches have disclosed a small piezonuclear fusion effect, it remains to disentangle the factors which influence the fusion rate.

While the need for off-equilibrium conditions is clearly implied by our data, techniques other than electrochemical may also be successful. We have begun to explore the use of ion implantation, and of elevated pressures and temperatures mimicking geological conditions. If deuteron-deuteron fusion can be catalyzed, then the d-t fusion reaction is probably favored due to its much larger nuclear cross section. Cold nuclear fusion in condensed matter may be of interest as a novel probe of metal-hydrogen systems, including geological ones, and as a source of monoenergetic neutrons. Furthermore, while the fusion rates observed so far are small, the discovery of cold nuclear fusion in condensed matter opens the possibility at least of a new path to fusion energy.

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References

1. Van Siclen, C. D. & Jones, S. E. *Journal of Physics G. Nucl. Phys.* **12**, 213-221 (1986).
2. Jones, S. E. *Nature* **321**, 127-133 (1986); Rafelski, J. & Jones, S. E. *Scientific American* **257**, 84-89 (July 1987).
3. Craig, H., Lupton, J. E., Welhan, J. A., & Poreda, R. *Geophys. Res. Lett.* **5**, 897 (1978); Lupton, J. E., & Craig, H. *Science* **214**, 13 (1981); Mamyrin, B. A. & Tolstikhin, L. N. *Helium Isotopes in Nature* (Elsevier, Amsterdam, 1984).
4. Craig, H. & Lupton, J. E., *The Oceanic Lithosphere: Vol. 7, The Sea*, Chap. 11 (John Wiley and Sons, New York, 1981)
5. Mamyrin, B. A., Khabarin, L. V. & Yudenich, V. S. *Sov. Phys. Dokl.* **23**, 581-583 (1978).
6. Jensen, G. L., Dixon, D. R., Bruening, K. & Czirr, J. B. *Nucl. Inst. and Methods* **220**, 406 (1984); and paper in preparation.
7. Hess, W. N., Patterson, H. W., and Wallace, R., *Phys. Rev.* **116**, 445 (1959).
8. MCNP: Monte Carlo Neutron and Photon Transport Code, CCC-200. Available from Radiation Shielding Information Center, Oak Ridge National Laboratory (Version 3).
9. Bowman, R. C. Jr. in *Metal Hydrides* (ed. G. Bambakides) pp. 109-144 (New York, Plenum, 1981).
10. Jones, S. E., et al. *Physical Review Letters* **51**, 1757-1760 (1983).

Figure Captions

Figure 1. Schematic set-up of the experiment. Electrolytic cells are shown on top of the neutron spectrometer.

Figure 2. Foreground (solid) and background (shaded) counts are shown as functions of pulse height in the neutron spectrometer. Note that ten counts have been added to each three-channel bin for clarity of presentation.

Figure 3. Difference spectrum obtained by substracting scaled background from the foreground. Statistical errors are shown for each eight-channel bin.

Figure 4. Ratio of foreground rate to background rate for each run, in the 2.5 MeV-energy region of the pulse-height spectrum. Statistical errors (± 1 σ) are shown.

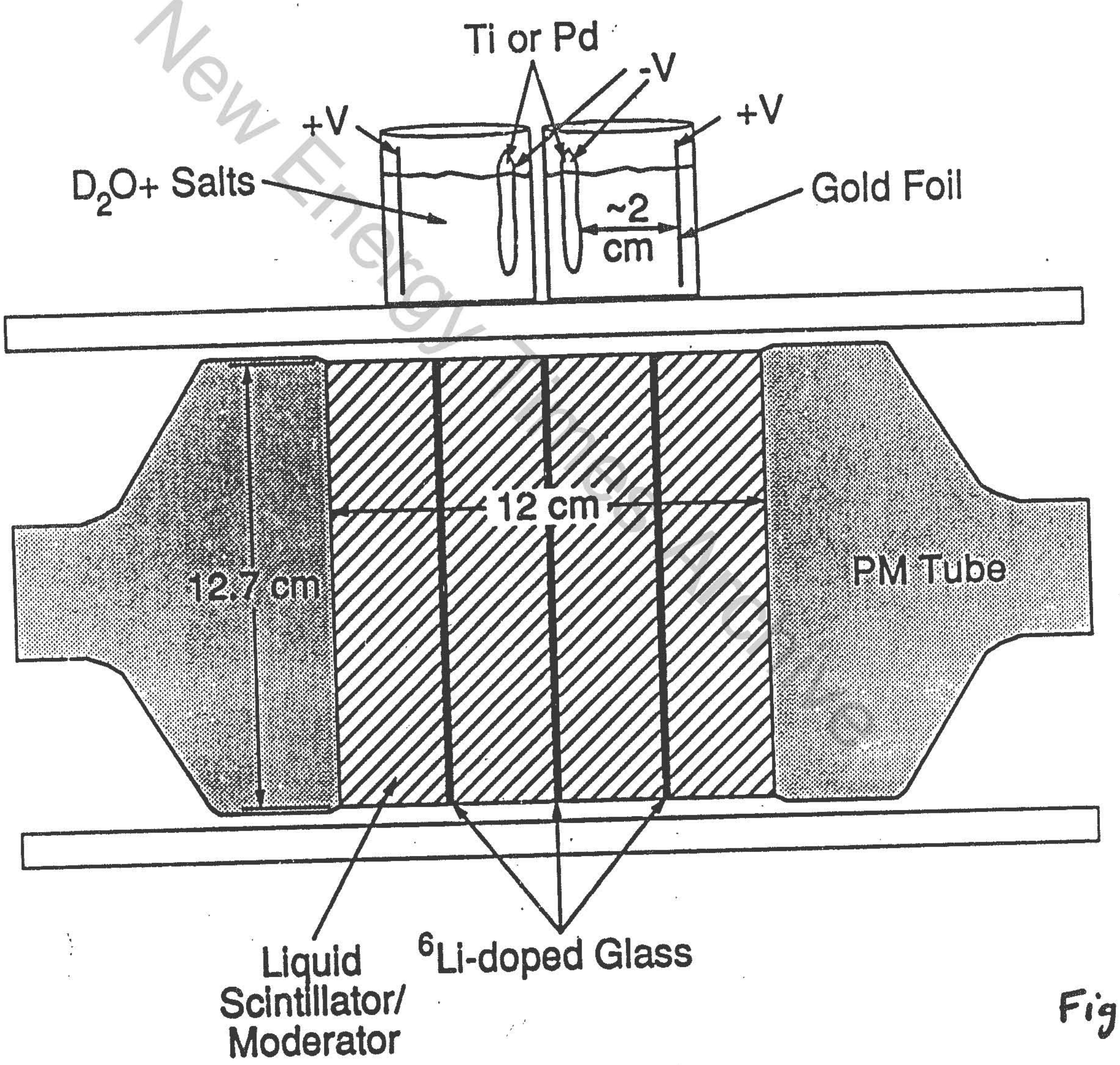
Ti or Pd
-V
+V
+V
D_2O+ Salts
Gold Foil
~2 cm
12 cm
12.7 cm
PM Tube
Liquid Scintillator/ Moderator
^{6}Li-doped Glass

Fig. 1

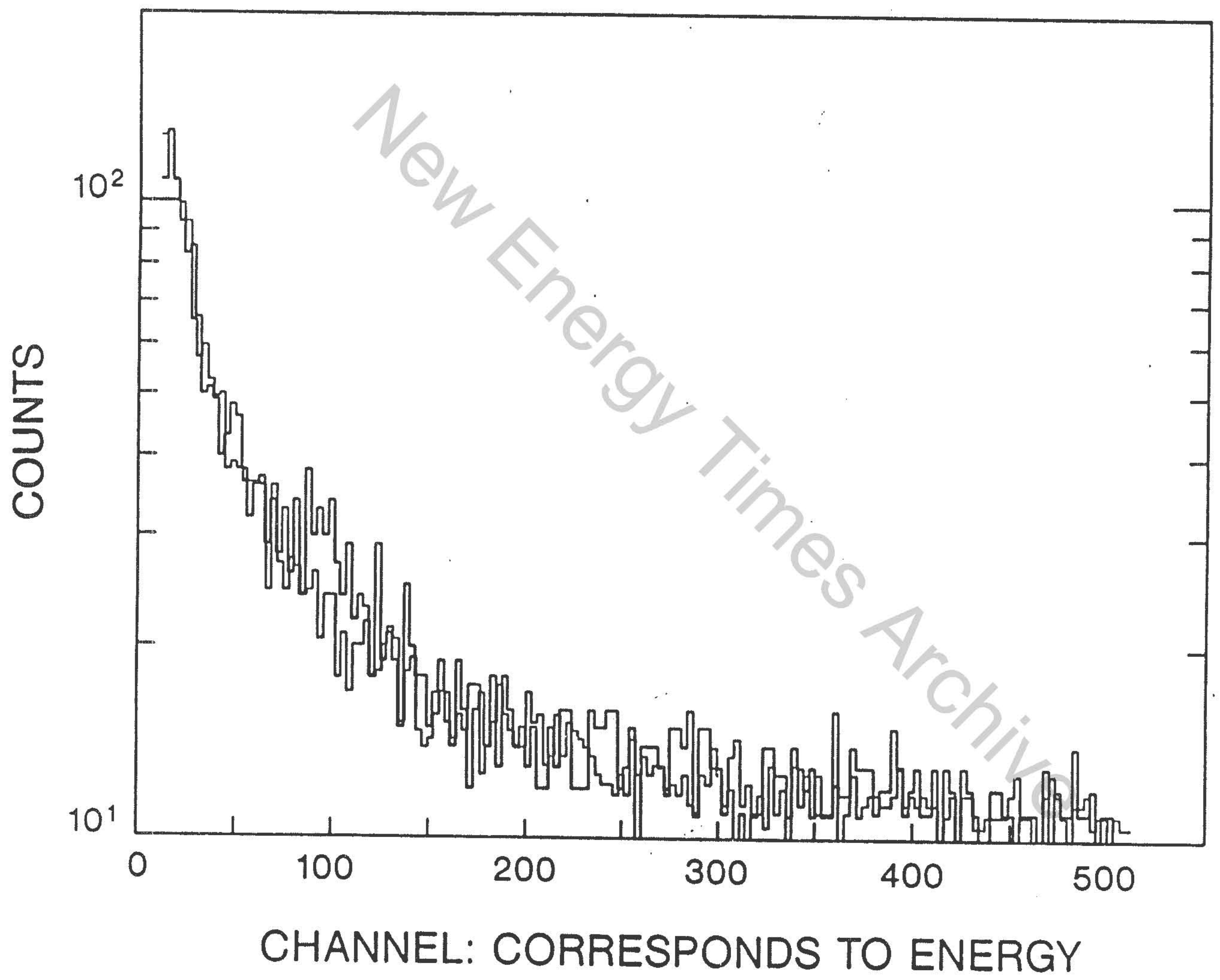
COUNTS
10^2
10^1
0
100
200
300
400
500
CHANNEL: CORRESPONDS TO ENERGY

Fig. 2

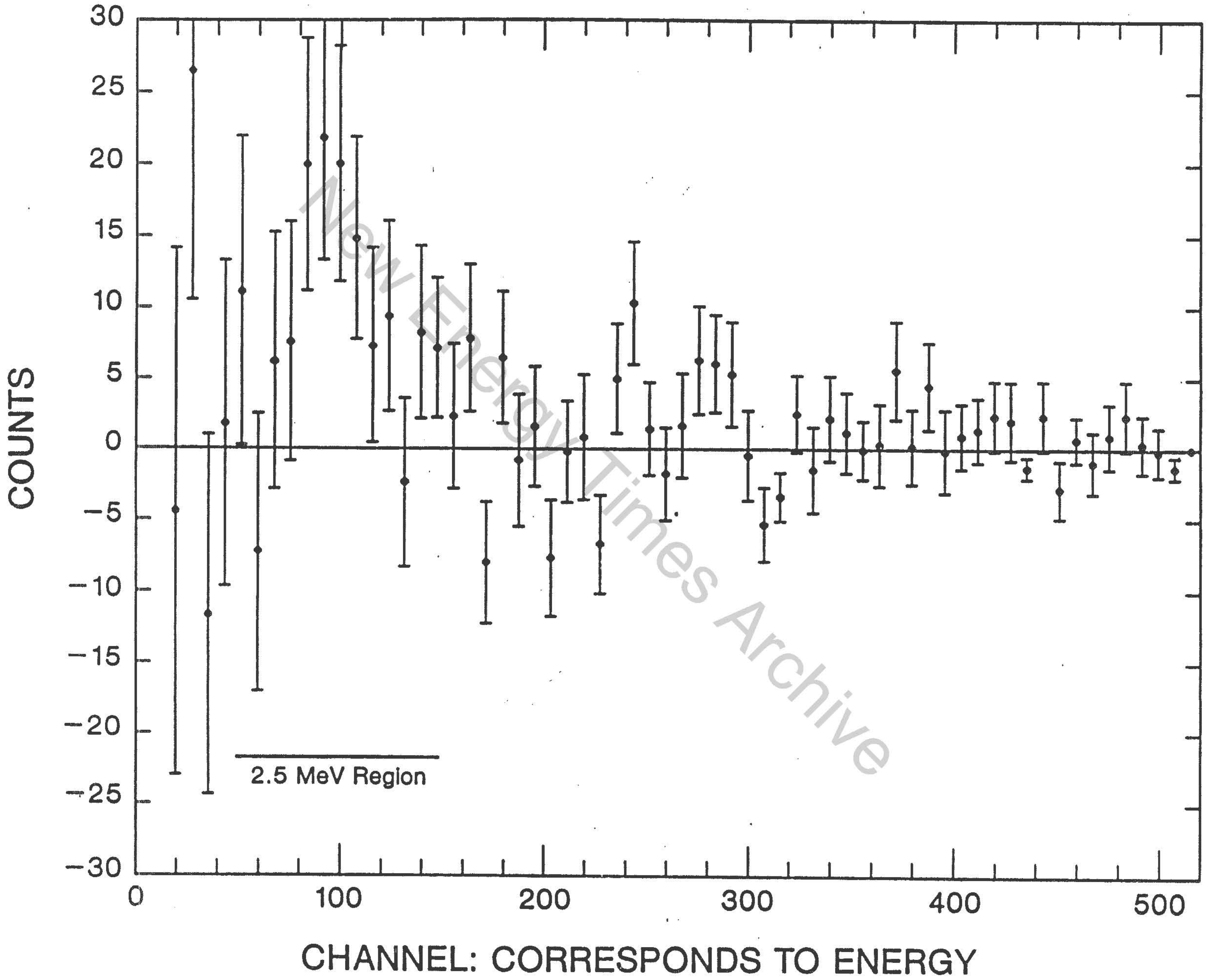
COUNTS
30
25
20
15
10
5
0
−5
−10
−15
−20
−25
−30
0
100
200
300
400
500
2.5 MeV Region
CHANNEL: CORRESPONDS TO ENERGY

Fig. 3

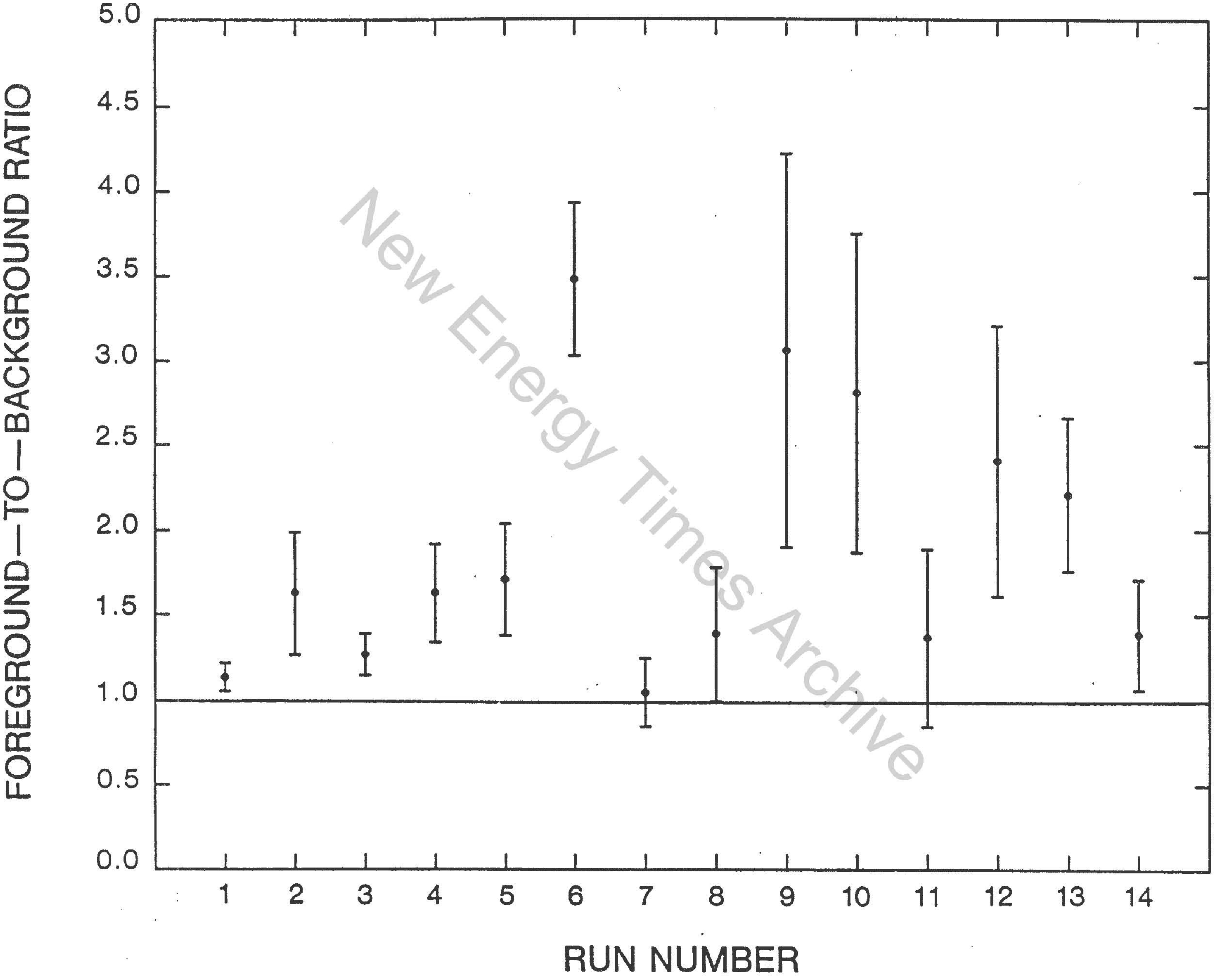
FOREGROUND—TO—BACKGROUND RATIO
5.0
4.5
4.0
3.5
3.0
2.5
2.0
1.5
1.0
0.5
0.0
1
2
3
4
5
6
7
8
9
10
11
12
13
14
RUN NUMBER

Fig. 4